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I. THE FUEL OF THE SUN.

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AT the suggestion of the editor, I offer to the readers of the "Quarterly Journal of Science" the following sketch of the main argument worked out more fully in the essay I have published under the above title, hoping that many who hesitate to plunge into a presumptuous speculative work of more than 200 octavo pages may read this article, and reflect upon the subject.

The book has been handled in a most courteous and indulgent spirit by all the reviewers who have noticed it, but none have ventured to grapple with the argument it contains, although every possible opportunity and provocation for doing so is designedly afforded. It all rests upon the question which is discussed in the first three chapters, viz., Whether the atmosphere which surrounds our earth is limited or unlimited in extent? If my reasoning upon this fundamental question is refuted, all that follows necessarily falls to the ground. If I am right, all our standard treatises on pneumatics and meteorology, which repeat the arguments contained in Dr. Wollaston's celebrated paper, must be remodelled. At the outset, I reprint that paper, and point out a very curious and monstrous fallacy which, for half a century, has remained undetected, and has been continually repeated. As the main point of issue between myself and Dr. Wollaston is merely a question of very simple arithmetic and geometry, nothing can be easier than to set me right if I am wrong; and, as the philosophical consequences depending upon this issue are of vast and fundamental importance, the question cannot be ignored by those who stand before the world as scientific authorities, without a practical abdication of their philosophical responsibilities. Any man who publishes an astronomical or meteorological treatise without discussing this question, which stands before him at the threshold of his subject, is unfit for the task he has undertaken, and unworthy of public confidence.

This may appear a strong conclusion just now, but a few years will be sufficient to graft it firmly into the growth of scientific public opinion.

"The Fuel of the Sun" is simply an attempt to trace some of the consequences which must of necessity result from the existence of an universal atmosphere, and it differs from other attempts to explain the great solar mystery, by making no demands whatever upon the imagination, *inventing* nothing,—no outside meteors, no new forces or materials. It supposes nothing whatever to exist but the known facts of the laboratory—the familiar materials of the earth and its atmosphere. It is shown that these materials and the forces residing within them must of necessity produce a sun, and manifest eternally all the observed solar phenomena, provided only they are aggregated in the quantities which our own central luminary presents, and are surrounded by attendant planets such as his. Nothing is assumed or taken for granted beyond the simple fundamental hypothesis that the laws of nature are uniform throughout the universe. The argument thus conducted leads us step by step to a natural, necessary, and connected explanation of the following important phenomena:—

1. The sources of solar and stellar heat and light.
2. The means by which the present amount of solar heat and light must be maintained so long as the solar system continues in existence.
3. The origin of the general and particular phenomena of the sun-spots.
4. The cause of the varying splendour of the photosphere, including such details as the "faculæ," "mottling," "granulations," &c., &c.
5. The forces which upheave the solar prominences.
6. The origin of the corona and zodiacal light.
7. The origin of the meteorites and the asteroids.
8. The meteorological phenomena of the planets.
9. The origin of the rings of Saturn.
10. The origin of the special structure of the nebulæ.
11. The source of terrestrial magnetism, and its connection with solar activity.

The first and second chapters are devoted to an examination of the limits of atmospheric expansibility. The experimental investigations of Dr. Andrews, Mr. Grove, Mr. Gassiot, and M. Geissler are cited to prove that the expansibility of the atmosphere is unlimited, and other cosmical evidence is adduced in support of this conclusion.

As this, which is really the foundation of the whole argument, is directly opposed to the views expressed by Dr. Wollaston, in his celebrated paper on "The Finite Extent of the Atmosphere," published in 1822, and generally accepted as established science, this paper is reprinted in the second chapter, and carefully examined.

Dr. Wollaston says "that air has been rarefied so as to sustain 1-100th of an inch of barometrical pressure," and further that "beyond this limit we are left to conjectures founded on the supposed divisibility of matter; if this be infinite, so also must be the extent of our atmosphere."

I contend that our knowledge of the whole subject is fundamentally altered since these words were written. We are no longer "left to conjectures founded on the supposed divisibility of matter" to determine the possibility of further expansibility than that indicated by 1-100th of an inch of barometrical pressure, as we now have means of obtaining ten times, a hundred times, a thousand times, or even an infinitely greater rarefaction than Wollaston's supposed limit, an absolute vacuum being now obtainable; and although the transmission of electricity affords a means of testing the existence of atmospheric matter with a degree of delicacy of which Wollaston had no conception, we are still unable to detect any indication of any limit to its expansibility.

The most remarkable part of Dr. Wollaston's paper is the *reductio ad absurdum* by which he seeks to finally demonstrate the finite extent of our atmosphere. He maintains, as I do, that if the elasticity of our atmosphere is unlimited, its extension must be commensurate with the universe, that every orb in space will, by gravitation, gather around itself an atmosphere proportionate to its gravitating power, and that, by taking the known quantity of the earth's atmosphere as our unit, we may calculate the amount of atmosphere possessed by any heavenly body of which the mass is known. On this basis, Dr. Wollaston calculates the atmosphere of the sun, and concludes that its extent will be so great as to visibly affect the apparent motions of Mercury and Venus, when their declination makes its nearest approach to that of the sun. No such disturbance being actually observable, he concludes that such an atmosphere as he has calculated cannot exist. In like manner he calculates the atmosphere of Jupiter, and finds it to be so great that its refraction would be sufficient "to render the fourth satellite visible to us when behind the centre of the planet, and consequently to make it appear on both (or all) sides at the same time."

On examining these calculations, I have discovered the very curious error above referred to. As this is a matter of figures that cannot be abridged, I must refer the reader to the original calculations. I will here merely state that the result of Wollaston's method of calculating the solar gravitation atmosphere and that of Jupiter and the moon is the monstrous conclusion that, in ascending from the surface of the given orb, we always have the same limited amount of atmospheric matter above as that with which we started, although we are continually leaving a portion of it below.

Wollaston's mistake is based on the assumption that, under the circumstances supposed, the atmospheric pressure and density, at any given distance from the centre of the given orb, will vary inversely with the square of the distance. As the area of the base upon which such pressure is exerted varies *directly* with the square of the distance, the total atmosphere above every imaginable starting-distance would thus be ever the same. That this assumption, so utterly at variance with the known laws of atmospheric distribution, should have remained unchallenged for half a century, and that the conclusions based upon it should be accepted by the whole scientific world, and repeated in all our standard treatises, is, I think, one of the most remarkable curiosities presented by the history of science. If it were merely a little cobweb in some obscure corner of philosophy, there would be nothing surprising in its escape from the besom of scientific criticism, but this is so far from being the case that it has hung, since 1822, like a dark veil obscuring another, a wider and most interesting view of the universe which the idea of an universal atmosphere opens out. But I must now proceed to the next stage of the argument.

Starting from the conclusion reached in the previous chapters, that the atmosphere of our earth is but a portion of an universal elastic medium which it has attached to itself by its gravitation, and that all the other orbs of space must, in like manner, have obtained their proportion, I take the earth's mass, and its known quantity of atmospheric envelope as units, and calculating, by the simple rule I have laid down in opposition to Wollaston's, I find that the total weight of the sun's atmosphere should be at least 117,681,623 times that of the earth's, and the pressure at its base equal, at least, to 15,233 atmospheres. What must be the results of such an atmospheric accumulation?

The experiment of compressing air in the condensing syringe, and thereby lighting a piece of German tinder, is familiar to all who have studied even the rudiments of physical science. Taking the formulæ of Leslie and Dalton, and applying them to the solar pressure of 15,233 atmospheres, we arrive, according to Leslie, at the inconceivable temperature of 380,832° C., or 685,529° F., as that due to this amount of compression, or, according to Dalton, at 761,665° F. What will be the effects of such a degree of heat upon materials similar to those of which our earth is composed?

Let us first take the case of water, which, for reasons I have stated, should be regarded as atmospheric, or universally diffused matter.

This brings us to a subject of the highest and widest philosophical and practical importance. I refer to the antagonism between the force of heat and that of chemical combination, to which the French chemists have given the name "dissociation." Having myself been unable to find any satisfactory English account of this subject at a time when it had already been well treated by French and German authors, in the form of published lectures and cyclopædia articles, I shall assume that others may have encountered a similar difficulty, and therefore dwell rather more fully upon this part of my present summary.

It appears that all chemical compounds may be decomposed by heat, and that, at a given pressure, there is a definite and special temperature at which the decomposition of each compound is effected. For the absolute and final establishment of the universality of this law further investigations are necessary, actual investigations having established it as far as they have gone, but these have not been exhaustive.

There appears to be a remarkable analogy between dissociation and evaporation. When a liquid is vaporised, a certain amount of heat is "rendered latent," and this quantity varies with the liquid and with the pressure, but is definite and invariable for each liquid at a given pressure. In like manner, when a compound is dissociated, a certain amount of heat is "rendered latent," or converted into dissociating force, and this varies with each compound and with the pressure, but is definite and invariable for each compound at a given pressure. Further, when condensation occurs, an amount of heat is evolved as temperature exactly equal to that which was rendered latent in evaporation of the same substance under the same pressure; and, in like manner, when chemical re-combination of dissociated elements occurs, an amount of heat is evolved as temperature exactly equal to that which disappeared when the compound was dissociated by heat *alone* under the same pressure.

According to the recently adopted figures of M. Deville, the temperature at which the vapour of water becomes dissociated under ordinary atmospheric pressure is 2800° C., and the quantity of heat which disappears as temperature in the course of dissociation is 2153 *calories*, *i.e.*, sufficient to raise 2153 times its own weight of *liquid* water 1° C.; but, as the specific heat of aqueous vapour is to that of liquid water as 0·475 to 1, the latent heat expressed in the temperature it would have given to aqueous vapour is = 4532° C., or 8158° F.

In order to render the analogy between the ebullition and dissociation of water more evident and intelligible, I will state it as follows:—

To commence the ebullition of water under ordinary pressure, a temperature of 100° C., or 212° F., must be attained.	To commence the dissociation of aqueous vapour under ordinary pressures, a temperature of 2800° C., or 5072° F., must be attained.
To complete the ebullition of a given quantity of water, an amount of heat must be applied, sufficient to have raised the water 537° C., or 968° F., above its boiling-point, had it not evaporated.	To complete the dissociation of a given quantity of aqueous vapour, an amount of heat must be applied sufficient to have raised the vapour 4532° C., or 8158° F., above its dissociation-point had it not decomposed.
In order that a given quantity of vapour of water shall condense, it must give off sufficient heat to raise its own weight of water 537° C., or 968° F.	In order that a given quantity of the elements of water may combine, they must give off sufficient heat to raise their own weight of aqueous vapour 4532° C., or 8158° F.

I have expressed these generalisations and analogies rather more definitely than they have been hitherto stated, but those who are acquainted with the researches of Deville, Cailletet, Bunsen, &c., will perceive that I am justified in doing so.

With the general laws of the dissociation of water thus before us, we may follow out the necessary results of the above-stated pressure and consequent evolution of heat in the lower regions of the solar atmosphere upon the large proportion of aqueous vapour which I have shown that it should contain.

It is evident that the first result will be separation of this water into its elements, accompanied with a loss of temperature corresponding to the latent heat of dissociation. We may assume that in the lower regions of the solar atmosphere the free heat evolved by mechanical compression will be more than sufficient to dissociate the whole of the aqueous vapour, and thus the dissociated gases will be left at a higher temperature than was necessary to effect their dissociation. Their condition will thus be analogous to that of superheated steam, they will have to give off some heat before they can *begin* to combine.

There will, however, be somewhere an elevation where the heat evolved by the joint compression of the elementary and combined gases will be just sufficient to dissociate the latter, and here will be the meeting surface of the combined and the uncombined constituents of water. There will be a sphere containing uncombined oxygen and hydrogen surrounded by an atmospheric envelope containing large quantities of aqueous vapour, and the temperature at this limiting surface will be just equal to that of the oxyhydrogen flame under a corresponding pressure.

What will occur under these conditions? Will the "detonating gases" behave as in the laboratory? Obviously not, as a glance at the third of the above parallel propositions will show. The dissociated gases cannot combine without giving off their 4532° of latent heat as actual temperature. This can only be effected by communication with matter which is cooler than itself.

If a bubble of steam is surrounded by water maintained at the boiling temperature, it will not condense at all, because any effort of condensation would be accompanied with an evolution of heat exactly sufficient to evaporate its own result. If, however, the surrounding water is slowly radiating, or otherwise losing its heat, the enclosed bubble of steam will condense proportionately by giving off to its envelope an amount of its latent heat just sufficient to maintain the water at the boiling-point.

For further illustration, let us conceive the case of a certain quantity of the elements of water heated exactly to the temperature of dissociation, and confined in a vessel the sides of which are maintained externally at precisely the same temperature as the gases within, so that no heat can be added or taken away from them. No sensible amount of combination could now take place, as the first infinitesimal effort of combustion, or combination, would set free just the amount of heat required to decompose its own result. Let us now suppose a modification of these conditions, viz., that the vessel containing the dissociated gases, at the temperature of dissociation, shall be surrounded with bodies cooler than itself, *i.e.*, capable of receiving more heat from it than they radiate towards it; there would then take place just so much combustion as would set free the amount of heat required to maintain the temperature of the vessel at the dissociation-point; or, in other words, combustion would go on to the extent of setting free just so much heat as the gaseous mass was capable of radiating, or otherwise transmitting to surrounding bodies; and this amount of combustion would continue till all the gases had combined.

We have only to give this hypothetical vessel a spherical form and an internal diameter of 853,380 miles,—to construct its enveloping sides of a thick shell of aqueous vapour, &c., and then, by placing in the midst of the contained dissociated gases a nucleus of some kind, we are hypothetically supplied with the main conditions which I suppose to exist in the sun.

A little reflection upon the application of the above-stated laws to these conditions will show that the stupendous ocean of explosive gases would constitute an enormous stock of fuel capable, by its combustion, of setting free exactly the same quantity of heat as had previously been converted into decomposing or separating force; the amount of combustion would always be limited by the possible amount of radiation, and the radiation would again be limited by the resisting envelope of aqueous vapour produced by this combustion.

If these conditions existed in a perfectly calm and undisturbed solar atmosphere, there would be a continually-increasing external envelope of aqueous vapour, and a continually-diminishing inner atmosphere of combustible gases; there would be a gradual diminution of the amount of solar radiation, and a slow and perpetually-retarding progress towards solar extinction.

It should be noted that, according to this explanation, the *supply* of heat is originally derived from atmospheric condensation due to gravitation, that the *storage* of surplus heat is effected by dissociation, and its *evolution* mainly by recombination or combustion.

The great difficulty, that of the perpetual renewal of the solar fuel, still remains unsolved; the fact that during the millions of years of geological history we find no indications of any declining average of solar energy is so far still unexplained by this, as by every other, attempt to account for the origin of solar and stellar light and heat.

In his inaugural address to the British Association Meeting of 1866, Mr. Grove put the following very suggestive question:—"Our sun, our earth, and planets are constantly radiating heat into space; so, in all probability, are the other suns, the stars, and their attendant planets. What becomes of the heat thus radiated into space? If the universe has no limit,—and it is difficult to conceive one,—there is a constant evolution of heat and light; and yet more is given off than is received by each cosmical body, for otherwise night would be as light and as warm as day. What becomes of the enormous force thus apparently non-recurrent in the same form?"

This is a grand question, a philosophical thought worthy of the author of "The Correlation of Physical Forces." Most philosophical thinkers will, I believe, agree with me in concluding that a sound reply to it will solve the great mystery of the everlasting radiations of our sun and all the other suns of the universe. So long as we regard these suns as the *sources* of continually-expended forces of light and heat, their everlasting and unabated renewal becomes a mystery utterly inscrutable to the human intellect, since the creation of new force, or any addition to the total forces of the universe, is as inconceivable to us as any addition to the total matter of the universe. The great solar question assumes a far more hopeful shape when we admit that all the forces of past radiations are somewhere diffused in space, and we ask whether a sun contains any mechanism by which it may collect and concentrate this diffused force, and thus perpetually gather from surrounding suns as much as it radiates towards them.

The next part of my work is an attempt to show that such a mechanism does exist in our solar system, and to explain its action.

We know that if atmospheric air is compressed it becomes heated, that if this heat is allowed to radiate and the air is again expanded to its original dimensions, it will be cooled below its original temperature to an extent precisely equal to the heat which it gave out when compressed. On this principle I endeavour to explain the everlasting maintenance of the solar and stellar radiations.

The sun is attended by his train of planets whose orbital motion he controls, but they in return react upon him as the moon does upon the earth. If this reaction were regular, like that of the moon upon the earth, a regular atmospheric tide would result; but the great irregularity of the dimensions, distances, and velocities of the planets produces a result equivalent to a number of clashing irregular tides in the solar atmosphere; or, otherwise stated, the centre of motion and centre of gravity of the whole system will be perpetually varying with the varying relative positions of the planets, and thus the solar nucleus and solar atmosphere will be subject to irregularities of motion, which, though very small relatively to the enormous magnitude of the sun, must be sufficient to produce mighty vortices, and thus effect a continual commingling between the outer and inner atmospheric strata.

It must be remembered that, according to the preceding, the inner or lower strata of the solar atmosphere should consist of our ordinary atmospheric mixture of oxygen and nitrogen, and the dissociated elements of water and carbonic acid, besides some of the more volatile elements of the solar nucleus. Outside of this there should be a boundary limit where the dissociated gases are combining as rapidly as their latent heat can be evolved by radiation; this will form a shell or sphere of flame,—the photosphere,—and above or beyond this will be the sphere of vapours resulting from this combustion, which, by their resistance to radiation, will limit the evolution of heat and consequent combustion.

Now the vortices above referred to will break through the shell of combustion, and drag down more or less of the outer vapour into the lower and hotter regions of dissociated gases.

As there can be no action without equal and contrary reaction, there can be no vortices, either in the solar atmosphere or a terrestrial stream, without corresponding upheavals. These upheavals will eject the lower dissociated gases more or less completely through the vapourous jacket which restrains their normal radiations, and, thus liberated, they will rush into combination with an explosive energy comparable to that which they display in our laboratories; not, however, with an instantaneous flash, but with a continuous rocket-like combustion, the rapidity of which will be determined by the possibility of radiation. The heat evolved by this combustion, acting simultaneously with the diminution of pressure, will effect a continually augmenting expansion of these upheaved gases, and as the rapidity of combustion will be accelerated in proportion to elevation above the restraining vapours, an outspreading, far in excess of that which would be due to the original upheaving force, it is to be expected.

The reader who is acquainted with the phenomena of the solar prominences will at once perceive how all these expectations are fulfilled by actual observations, especially by the more recent observations of Zollner, Secchi, &c. I need scarcely add that the clashing tide-waves are the *faculæ*, and the vortices the sun-spots.

My present business, however, is to show how these vortices and eruptions—this down-rush in one part of the solar atmosphere and up-rush in another—contribute to the permanent maintenance of the solar light and heat. It must be understood that these outbursts are only visible to us as luminous prominences during the period of their explosive outburst, and while still subject to great expansive tension. Long after they have ceased to be visible to us their expansion must continue, until they finally and fully mingle with the medium into which they are flung, and attain a corresponding degree of rarefaction. This must occur at thousands or tens of thousands of miles above the photosphere, according to the magnitude of the ejection. The spectroscopic researches of Frankland and Lockyer have shown that the atmospheric pressure at about the outer surface of the photosphere does not exceed that of our atmosphere, and as we consider that at 100 miles above this we reach the ethereal regions, I may safely regard all the upper portion of these solar ejections as having left the solar atmosphere proper, and become commingled with the general interstellar medium.

If the sun were stationary, or merely rotating, in the midst of this universal atmosphere, the same material that is ejected to-day would in the course of time return, and be whirled into the great sun-spot eddies; but such is not the case; the sun is driving through the ether with a velocity of about 450,000 miles per twenty-four hours.

What must be the consequence of this motion? The sun will carry its own special atmospheric matter with it; but it cannot thus carry the whole of the interstellar medium. There must be a limit, graduated no doubt, but still a practical limit, at which its own atmosphere will leave behind, or pass through, the general atmospheric matter. There must be a heaping or condensation of this matter in the front, a rarefaction or wake in the rear, and a continuous flow of newly encountered atmosphere around the boundaries in the opposite direction to that of the sun's motion. The result of this must be that a great portion of the ejected atmospheric matter of the prominences will be swept permanently to the rear, and its place supplied by the material occupying the space into which the sun is advancing. We are thus presented with a mighty machinery of solar respiration; some of this newly arriving atmospheric matter must be stirred into the vortices, its quantity being exactly equivalent to that of the old material expired by the explosive eruptions, and left in the rear.

Now, the new atmospheric matter which is thus encountered and inspired, is the recipient of the everlasting radiations whose destination is the subject of Mr. Grove's enquiry; and these, when thus encountered and compressed, will of necessity evolve more or less of the heat which, through millions of millions of centuries they have been gradually absorbing; while, on the other hand, the expired or ejected matter of the gaseous eruptions will, like the artificially compressed air above referred to, have lost all the heat which during its solar existence it had by compression, dissociation, and re-combination, contributed to the solar radiations. Therefore, when again fully expanded, it will be cooler than the general medium from which it was inspired by the advancing sun.

The daily supply of fresh atmospheric fuel will be a cylinder of ether of the same diameter as the sun and 450,000 miles in length! I have calculated the weight of this cylinder of ether on the assumption (which of course is purely arbitrary) that the density of the interstellar medium is one ten-thousandth part of that of our atmosphere. According to this its weight would be 14,313,915,000,000,000,000 tons, affording a supply of 165 millions of millions of tons per second; or, if we assume the interstellar medium to have a density of only one millionth of that of our atmosphere, the supply would be rather more than one and a half millions of millions of tons per second. The proportion of this which is effective in the manner above stated is that which becomes stirred into the lower regions of the sun in exchange for the ejected matter of the prominences.

I will not here dwell upon the bombardment hypothesis, beyond observing that my explanation of solar phenomena supplies a continuous bombardment of the above stated magnitude without adding anything to the magnitude of the sun.

So far, then, I answer Mr. Grove's question, by showing that the heat radiated into space by each of the solid orbs that people its profundities, is received by the universal atmospheric medium; is gathered again by the breathing of wandering suns, who inspire as they advance the breath of universal heat and light and life; then by impact, compression, and radiation, they concentrate and re-distribute its vitalising power; and after its work is done, expire it in the broad wake of their retreat, leaving a track of cool exhausted ether—the ash-pits of the solar furnaces—to reabsorb the general radiations, and thus maintain the eternal round of life.

But ere this, a great difficulty has probably presented itself to the mind of the reader. He will refer to the calculations that have been made in order to determine the actual temperature of the solar surface and the intensity of its luminosity. Both of these are vastly in excess of those obtained in our laboratory experiments by the combustion of the elements of water. Even taking into consideration the dissociated carbonic acid whose elements should be burning in the photosphere with those of water, and adding to these the volatile metals of the solar nucleus whose dissociated vapours must, under the circumstances stated, be commingled with those of the solar atmosphere, and therefore contribute to the luminosity by their combustion, still by burning here on the earth a jet of such mixed gases and vapours we should not obtain any approach to either the luminosity or the temperature which is usually attributed to the sun.

I have made a few very simple experiments, the results of which I think remove entirely these difficulties. They were made with the assistance of Mr. Jonathan Wilkinson, the official gas examiner to the Sheffield Corporation, using his photometric and gas measuring apparatus. We first determined the amount of light radiated by a single fish-tail gas-burner consuming a measured quantity of gas per hour. We found that when another was placed behind this, so that all the light of the second had to pass through the first, that the light of the two (measured by the illuminating intensity of their radiations upon a screen just as the solar luminosity has been measured) was just double that of one flame, three flames (still presenting to the photometric screen only the surface of one) gave it three times the amount of illumination, and so on with any number of flames we were able to test. Mr. Wilkinson has since arranged 100 flames on the same principle, *i.e.*, so that the 99 hinder flames shall all radiate through the one presented to the screen, thus affording the same surface as a single flame, but having 100 times its *thickness* or *depth*, and he finds that the law indicated by our first experiments is fully verified; that the 100 flames thus arranged illuminate the screen 100 times as intensely as the single flame. Other modifications of these experiments described in chapter 7 of "The Fuel of the Sun," establish the principle that a common hydrocarbon gas flame is transparent to its own radiations, or in other words, that the amount of light radiated from such a flame, and its apparent intensity of luminosity, is proportionate to its thickness; therefore the luminosity of the sun may be produced by a photosphere having no greater intrinsic brilliancy than the flame of a tallow candle, provided the flame is of sufficient depth or thickness. I see good reasons for inferring that its intrinsic brilliancy is less than that of a candle—somewhere between that and a Bunsen's burner.

I made a similar series of experiments upon the radiation of the *heat* of flames through each other, and arrived at similar results; but my apparatus in these experiments was not so delicate and reliable as in the experiments on light, and, therefore, I cannot so decidedly affirm the absolute diathermacy of flame to its own radiations. Within the limits of error of these experiments, I found that with the same radiant surface presented to the thermometer, every addition to the thickness of the flame produced a proportionate increase of radiation.

This important law, though hitherto unnoticed by philosophers, is practically understood and acted upon by workmen who are engaged in furnace operations. Present space will not permit me to illustrate this by examples, but in passing I may mention the "mill furnaces," where armour plates and other large masses of iron are raised to a welding temperature by radiant heat, and the ordinary puddling furnace, where iron is melted by radiant heat. In both of these special arrangements are made to obtain a "body" or thickness of radiant flame, while *intensity* of combustion is neglected and even carefully avoided.

According to this there are two factors engaged in producing the radiant effect from a given surface, *intensity* and *quantity*, *i.e.*, *brilliancy* and *thickness* in the case of light, and *temperature* and *thickness* in the case of heat. In the Bude light, for example, consisting of concentric rings of coal gas we have small intensity with great quantity, in the lime light we have a mere surface of great brilliancy but no thickness. If I am right the surface of the moon may be brighter than the luminous surface of the sun, the peculiarities of moonlight depending upon intensity, those of sunlight upon quantity of light.

The flame that roars from the mouth of a Bessemer converter has but small intrinsic brilliancy, far less than that of an ordinary gas flame, as may be seen by observing the thin waifs that sometimes project beyond the body of the flame. Nevertheless, its radiations are so effective that it is a painfully dazzling object even in the midst of sunny daylight; but then we have here not a hollow flame fed only by outside oxygen, but s solid body of flame several feet in thickness. Even the pallid carbonic acid flame which accompanies the pouring of the spiegeleisen has marvellous illuminating power.

The reader will now be able to understand my explanation of the sun-spots, of their nucleus, umbra, and penumbra. From what I have stated respecting the planetary disturbances of the solar rotation, the photosphere should present all the appearances due to the movements of a fiery ocean, raging and seething in the maddest conceivable fury of perpetual tempest. If the surface of a river flowing peacefully between its banks is perforated with conical eddies whenever it meets with a projecting rock or obstacle, or other agency which disturbs the regularity of its course, what must be the magnitude of the eddies in this ocean of flame and heated gases, when stirred to the lowest depths of its vast profundity by the irregular reeling of the solar nucleus within. Obviously, nothing less than the sun-spots; those mighty maelströms into which a world might be dropped like a pea into an egg-cup.

When the photosphere or shell of combining gases is thus ripped open, the telescopic observer looks down the vortex, which, if deep enough, reveals to him the inner region of dissociated gases and vapours. But these have the opposite property to that which I have shown to belong to flame; they are opaque to their own special radiations, while the flame is transparent to the light of the inner portions of itself. Thus, the dissociated interior of the solar envelope, though absolutely white-hot, will be comparatively dark (direct experiment has proved that the darkness of the spots is only relative).

The sides of the vortex funnel will consist of a mixture of dissociated gases, flaming gases, and combined gases, and will thus present various thicknesses of flame, and thereby effect the various shades of the penumbra. Space will not permit me here to follow up the details of this subject, as I have done in the original work, where it is shown that if the telescope had not yet been invented, all the telescopic details of spot phenomena might have been described *à priori* as necessary consequences of the constitution I have above ascribed to the sun.

Not merely the great spot phenomena, but all the minor irregularities of the photosphere follow with similarly demonstrable necessity. Thus the many interfering solar tides must throw up great waves, literally mountainous in their magnitude, the summits and ridges of which, being raised into a higher region of the absorbing vaporous atmosphere that envelopes the photosphere, will radiate more freely, its dissociated matter will combine more abundantly, and will thicken the photosphere immediately below; this thicker flame will be more luminous than the normal surface, and thus produce the phenomena of the *faculæ*.

Besides these great ground-swells of the flaming ocean of the photosphere, there must be lesser billows, and ripples upon these, and mountain tongues of flame all over the surface. The crests of these waves, and the summits of these flame-alps, presenting to the terrestrial observer a greater depth of flaming matter, must be brighter than the hollows and valleys between; and their splendour must be further increased by the fact, that such upper ridges and summits are less deeply immersed in the outer ocean of absorbing vapours, which limits the radiation of the light as well as the heat of the photosphere. The effect of looking upon the surface of such a wild fury of troubled flame, with its confused intermingling of gradations of luminosity, must be very puzzling and difficult to describe; and hence the "willow leaves," "rice grains," "mottling," "granules," "things," "flocculi," "bits of white thread," "cumuli of cotton wool," "excessively minute fragments of porcelain," "untidy circular masses," "ridges," "waves," "hill knolls," &c., &c., to which the luminous irregularities have been compared.

At the time I wrote, the means of examination of the edge of the sun by the spectroscope was but newly discovered, and the results then published referred chiefly to the prominences proper. Since that, a new term has been introduced to solar technology, the "sierra," and the observations of the actual appearances of this sierra precisely correspond to my theoretical description of the limiting surface of the photosphere, which was written before I was acquainted with these observed facts. This will be seen by reference to chapter 10, the subject of which is, "The Varying Splendour of Different Portions of the Photosphere."

But I must not linger any further upon this part of the subject, but proceed to another, where subsequent discoveries have strongly confirmed my speculations.

The mean specific gravity of the sun is not quite 1½ times that of water. The vapours of nickel, cobalt, copper, iron, chromium, manganese, titanium, zinc, cadmium, aluminium, magnesium, barium, strontium, calcium, and sodium have been shown by the spectroscope to be floating on the outer regions of the sun. None of these could constitute the body of the sun in a solid or liquid state, and subject to the enormous pressure which such a mass must exert upon itself without raising the mean specific gravity vastly above this; nor is there any other kind of matter with which we are acquainted which could exist within so large a mass in a liquid or solid state, and retain so low a density.

I must confess that my faith in the logical acumen of mathematicians has been rudely shaken by the manner in which eminent astronomers have described the umbra or nucleus of the sun-spots as the solid body of the sun seen through his luminous atmosphere, and the solid surface of Jupiter seen through his belts, and have discussed the habitability of Jupiter, Saturn, Uranus, and Neptune always on the assumption of their solidity, while the specific gravity of all of these renders this surface solidity a demonstrable physical impossibility.

If the sun (or either of these planets) has a solid or liquid nucleus, it must be a mere kernel in the centre of a huge orb of gaseous matter, and though I have spoken rather definitely of the solar atmosphere in order to avoid complication, I must not, therefore, be understood to suppose that there exists in the sun any such definite boundary to the base of the atmospheric matter as we find here on the earth. The temperature, the density, and all we know of the chemistry of the sun justify the conclusion that in its outer regions, to a considerable depth below the photosphere, there must be a commingling of the atmospheric matter with the vapours of the metals whose existence the spectroscope has revealed. Some of these must be upheaved together with the dissociated elements of water. They are all combustible, and, with a few exceptions, the products of their combustion would solidify after they were projected beyond the photosphere. Much of the iron, nickel, cobalt, and copper might pass through the fiery ordeal of such projection, and solidify without oxidation, especially when more or less enveloped in uncombined hydrogen.

It is obvious that, under these circumstances, there must occur a series of precipitations analogous to those from the aqueous vapour of our atmosphere. These gaseous metals, or their oxides, must be condensed as clouds, rain, snow, and hail, according to their boiling and melting points, and the conditions of their ejection. We know that sudden and violent atmospheric disturbance, accompanied with fierce electrical discharges, especially favour the formation of hailstones in our terrestrial atmosphere. All such violence must be displayed on a hugely exaggerated scale in the solar outbursts, and therefore the hailstone formation should preponderate, especially as the metallic vapours condense more rapidly than those of water on account of the much smaller amount of their specific heat and of the latent heat of their vapours.

What will become of these volleys of solid matter thus ejected with the furious and protracted explosions forming the solar prominences? In order to answer this question, we must remember that the spectroscope, as recently applied, merely displays the gaseous, chiefly the hydrogen, ejections; that these great gaseous flames bear a similar relation to the solid projectiles that the flash of a gun does to the grape-shot or cannon-ball. Mr. Lockyer says: "In one instance, I saw a prominence 27,000 miles high change enormously in the space of ten minutes; and, lately, I have seen prominences much higher born and die in an hour." He has recently measured an actual velocity of 120 miles per second in the movements of this *gaseous* matter of the solar eruptions, the initial velocity of which must have been much greater. If such is the velocity of the gaseous ejections, what must be that of the solid projectiles, and where must they go? A cosmical cannonade thus follows as a necessary result of the conditions I have sketched, and as prominence-ejections of greater or lesser magnitude are continually in progress, there must be a continual outpouring from the sun of solid fragments, which must be flung far beyond the limits of the gaseous prominences. As the luminosity of these glowing particles must be very small compared with that of the photosphere, they will be invisible in the glare of ordinary sunshine, but if our eyes be protected from this, they may then be rendered visible, both by their own glow and the solar light they are capable of reflecting. They should be seen during a total eclipse, and should exhibit radiant streams proceeding irregularly from different parts of the sun, but most abundantly from the neighbourhood of the spot regions. As these spot-regions occupy the intermediate latitudes between the poles and the equator of the sun, the greatest extensions of these outstreamings should be N.E. and S.W., and S.E. and N.W., while to the N., S., E., and W.—that is, opposite the poles and equator of the sun—there should be a lesser extension. The result of this must be an approximation to a quadrilateral figure, the diagonals of which should extend in a N.E. and S.W., and a S.E. and N.W. direction, or thereabouts. I say "thereabouts," because the zone of greatest activity is not exactly intermediate between the poles and the equator, but lies nearer to the solar equator.

Examined with the polariscope, these radiant streams should display a mixture of reflected light and self-luminosity. Examined with the spectroscope, a faint continuous spectrum due to such luminosity of solid particles should be exhibited, with possibly a few doubtful lines due to the small amount of vapour which, in their glowing condition, they might still give off. Besides this, there should appear the spectroscope indications due to violent electrical discharges, which must occur as a necessary concomitant of the furious ejections of aqueous vapour and solid particles. All these metallic hailstones must be highly charged, like the particles of vesicular vapour ejected from the hydro-electric machine, or the vapours and projectiles of a terrestrial volcanic eruption.

I need scarcely add that this exactly describes the actually-observed results of the recent observations on the corona, and that all the phenomena of this great solar mystery are but necessary and predicable results of the constitution I ascribe to the sun.

There is a method of manufacturing hypotheses which has become rather prevalent of late, especially among mathematicians, who take observed phenomena, and then arbitrarily and purely from the raw material of their own imagination construct explanatory atoms, media, and actions, which are shaved and pared, scraped and patched, lengthened and shortened, thickened and narrowed, till they are made to fit the phenomena with mathematical accuracy. These laborious creations are then put forth as philosophical truths, and, *afterwards*, the accuracy of their fitting to the phenomena is quoted as evidence of the positive reality of the ethers, atoms, undulations, gyrations, collisions, or whatever else the mathematician may have thus skilfully created and fitted. It appears to me that such fitness only proves the ingenuity of the fitter,—the skill of the mathematician,—and that all such hypotheses belong to the poetry of science; they should be distinctly labelled as products of mathematical imagination, and nowise be confounded with objective natural truths. Such products of the imagination of the expert may assist the imagination of the student in comprehending some phenomena, just as "Jack Frost" and "Billy Wind" may represent certain natural forces to babies; but if Jack Frost, Billy Wind, electric and magnetic fluids, ultimate atoms, interatomic ethers, nervous fluids, &c., are allowed to invade the intellect, and are accepted as actual physical existences, they become very mischievous philosophical superstitions.

I make this digression in order to repudiate any participation in this kind of speculation. Though "The Fuel of the Sun" is avowedly a very bold attempt to unravel majestic mysteries, I have not sought *to elucidate the known by means of the unknown*, as do these inventors of imaginary agents, but have scrupulously followed the opposite principle. I have invented nothing, but have started from the experimental facts of the laboratory, the demonstrated laws of physical action, and have followed up step by step what I understand to be the necessary consequences of these. Many years ago, I convinced myself that our atmosphere is but a portion of universal atmospheric matter, that Dr. Wollaston was wrong, and that the compression of this universal atmospheric matter is possibly the source of solar light and heat; but as this was long before M. Deville had investigated the subject of dissociation by heat,* I was unable to work out the problem at all satisfactorily. When I subsequently resumed the subject, I knew nothing about the corona, and had only read of the "red prominences" as possible lunar appendages, or solar clouds, or optical illusions. I had worked out the necessity of the gaseous eruptions, and their action in effecting an interchange of solar and general atmospheric matter, as the means of maintaining the solar light and heat, with no idea of proceeding further with the problem, when the announcement that the prominences were not merely unquestionable solar appendages, but were actually upheaved mountains of glowing hydrogen, suddenly and unexpectedly suggested their identity with my required atmospheric upheavals. It is true that their observed magnitude far exceeded my theoretical anticipations, and in this respect I have made some *à posteriori* adaptations, especially with the aid of a clearer understanding of the laws of dissociation which almost simultaneously became attainable.

In like manner, the necessity of the solid ejections presented themselves before I knew anything of the recently-discovered details of the coronal phenomena—when I had merely read of a luminous halo which had been seen around the sun, and vaguely supposed it to be due to some sort of atmospheric illumination. I inferred that streams of solid particles must be pouring from the sun, and showering back again, but had no idea that such streams and showers were actually visible until I was rather startled at finding the corona, instead of being, as I had rather loosely supposed, a mere uniform filmy halo, had been described by Mr. De la Rue in his Bakerian Lecture on the Eclipse of 1860, as "softening off with very irregular outline, and sending off some *long streams*," &c. I was then living on the sides of a Welsh mountain far away from public libraries, and being no astronomer, my own books kept me better acquainted with the current progress of experimental than with astronomical science.

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* My first memorandum on the subject is dated 23rd April, 1840, in a "Register of Ideas," then commenced in very early student days.

Even when "The Fuel of the Sun" was published I knew nothing of the American observations of the quadrangular figure of the corona, or should certainly have then quoted them, nor of the fact revealed by the Eclipse of December, 1870, that, "wherever on the solar disc a large group of prominences was seen on Mr. Seabroke's map, there a corresponding bulging out of the corona was chronicled on Professor Watson's drawing; and at the positions where no prominences presented themselves, there the bright portions of the corona extended to the smallest distances from the sun's limb;" and that Mr. Brothers's photographs *all* show the corona extending much farther towards the west than towards the east, the west being "the region richest in solar prominences." I am sorry that the limits of this paper will not permit me to enter more fully into the bearings of the recent studies of the corona and the prominences upon my explanations of solar phenomena, especially as the differences between the inner and outer corona which still appears to puzzle astronomers are exactly what my explanation demands. I must make this the subject of a separate paper, and proceed at once to the next step of the general argument.

Assuming that such ejections of solid matter are poured from the prominences, to what distances may they travel? In attempting to answer this question, I confessedly ventured upon dangerous ground, for at the time I wrote I only knew that the force of upheaval of the prominences must be enormous, *probably* sufficient to eject solid matter beyond the orbit of the earth and even beyond that of Mars. Actual measurements of the eruptive velocity of the solar prominences have since been made, and they are so great as to relieve me of my quantitative difficulty, and show that I was quite justified in the bold inference that these eruptions may account for the zodiacal light, the zones of meteors into which our earth is sometimes plunged, and even the outer zone of larger bodies, the asteroids.

But how, the reader will ask, can such solids, ejected from the sun, acquire orbital paths around him. "We have been taught that the parabola is the necessary path of such ejections." Mr. Proctor has evidently reasoned in this manner, for in last April number of "Fraser's Magazine" he says, that some of my ideas are "opposed to any known laws, physical or dynamical," that "there is nothing absolutely incredible in the conception, that masses of gaseous, liquid, or solid matter should be flung to a height exceeding manifold that of the loftiest of the coloured prominences; whereas it is not only incredible, but impossible, that such matter should in any case come to circle in a closed orbit round the sun."

More careful reading would have shown Mr. Proctor that I have considered other conditions besides those of the text-books, that the case is by no means one of simple radial projection from a fixed body into free space and undisturbed return. I have distinctly stated that "the recent ejections may have any form of orbit within the boundaries of the conic sections," from a straight line returning upon itself, due to absolutely vertical projection, to a circular orbit produced by the tangental projection of such curving prominences *as the ram's horn*, &c. The outline of the zodiacal light would be formed by the termination or aphelion portion of these excursions, or of such a number of them as should be sufficient to produce a visible result."

Again, speaking of the asteroids, in Chapter 14 I state that "I should have expected a still greater elongation and eccentricity in some of them, and such orbits may have existed; but an asteroid with an orbit of cometary eccentricity that would in the course of each revolution cross the paths of Mercury, Venus, the Earth, and Mars, in nearly the same plane, and dive through the thickly scattered zodiacal cluster, both in going to the sun and returning from it, would be subject to disturbances which would continue until one of two things occurred. Its tangental force might become so far neutralised and its orbit so much elongated, that finally its perihelion distance should not exceed the solar radius, when it would finish its course by returning to the sun. On the other hand, its tangental velocity might be increased by heavy pulls from Jupiter, when slowly turning its aphelion path, and be similarly influenced by friendly jerks in crossing the orbits of the inferior planets; and thus its orbit might be widened, until it ceased periodically to cross the path of any of the planets by establishing itself in an orbit constantly intermediate between any two. Having once settled into such a path, it would remain there with comparative stability and permanency. If I am right in this view of the dynamical history of these older ejections, all the long elliptical paths of zodiacal particles, meteorites, or asteroids, would thus in the course of ages become eliminated, and the remaining orbits would be of planetary rather than cometary proportions."

A little reflection on the above-stated laws of dissociation will show that the maximum violence of hydrogen explosion will not occur at the birth of the ejections, but afterwards, when the dissociated gases have been already hurled beyond the sphere of restraining vapours. If my explanation is correct, the typical form of a solar prominence should be that of a spreading tree with a tall stem. At first the least resistance to radiation and consequent explosive combination must be in the vertical direction, as this will afford the shortest line that can be drawn through the thickness of the surrounding jacket of resisting vapour; but when raised above this envelope, the dissociated gases cooled by their own expansion and comparatively free to radiate in all directions except downwards, will explode laterally as well as vertically, and thus spread out into a head. My theoretical prominence will be, in short, a monster rocket proceeding steadily upwards to a certain extent, and then bursting and projecting its missiles in every direction from the vertical to the absolutely horizontal. Should the latter acquire a velocity of about 300 miles per second, not merely a closed but even an absolutely circular orbit would be possible. These and the multitude of weaker lateral ejections, reaching the sun by short parabolic paths, explain the mystery of the inner corona.

I need only refer Mr. Proctor to his own recently published book on the Sun, where he will find on plates 4, 5, and 6, a number of drawings from Zöllner and Respighi, which so thoroughly confirm my necessary theoretical deductions that they might be a series of fancy sketches of my own. When we consider that the base of a prominence is only visible when it happens to start exactly from the limb of the sun, while the vastly greater proportion of those which are observed and have been drawn, have much of the stem cut off from view by the solar rotundity, the evidence afforded by such drawings in support of my theoretical deduction, that the typical form of the solar prominences is that of a palm-tree or bursting rocket, is greatly strengthened.*

In a paper by P. Secchi, dated Rome, March 20, 1871, and published in the "Comptes Rendus," March 27, this veteran solar observer speaks of the prominences as composed of jets, which, "upon reaching a certain elevation, stop and whirl upon themselves, giving birth to a brilliant cloud." This cloud is represented as spreading out on all sides from the summit of the combined jets. Again he says, "It is very common to see a little jet stop at a certain elevation above the chromosphere, and there spread itself out into a *wide hat* ('*un large chapeau*') of an absolutely nebulous constitution." This outspreading nebulosity is the flash of the incandescent vapours of the explosion which is theoretically demanded, by my explanation of the constitution of the sun, to occur exactly in the manner and place described. These will be rendered visible by the spectroscopic dilution of the continuous spectrum, while the solid projectiles that must proceed from them in every direction can only be seen during a solar eclipse.

The observations and drawing of Zöllner and Respighi were, for the most part, made while my book was in the press, and like those of Secchi above quoted, were unknown to me when I wrote, and I was then only able to quote, in support of my theoretical requirements, the evidences of actually observed tangental ejection afforded by Sir John Herschel's account of the great solar storm of September 1, 1859; but the special prominence which I have given to this and the rate of *horizontal motion* there measured, should have prevented Mr. Proctor from making the mistake he has in the article above referred to.

Besides this direct tangental projection there are other elements of motion contributing to the same result, such as the whirling of the prominences on themselves, their motion of translation on the sun's disc, and the rotation of the sun itself.

I must now bring this sketch to a close by stating that in order to submit the fundamental question of an universal atmosphere to an *experimentum crucis* analogous to that by which Pascal tested the atmospheric theory of Torricelli, I have calculated the theoretical density of the atmosphere of the moon and of each of the planets, and compared the results as severely as I could with the observed facts. As Jupiter is 27,100 times heavier than the moon, and between these wide extremes there are six planets presenting great variations of mass, the probabilities of accidental coincidence are overwhelmingly against me, and a close concurrence of observed telescopic refraction and other phenomena with the theoretical atmospheric density must afford the strongest possible confirmation of the soundness of the basis of my whole argument. Such a concurrence exists, and some new and very curious light is unexpectedly thrown upon the meteorology of Mars and the constitution of the larger planets; the latter, if I am right, must be miniature suns, *permanently* red- or white-hot, must have something like a photosphere, surrounded by a sphere of vapour (the outside of which we see), must have mimic spot vortices and prominences, and in the case of Saturn must eject volleys of meteoric matter, some of which should finally settle down into orbital paths, and thus produce the rings.

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* Any reader of "The Fuel of the Sun" will perceive that the vaporous envelope which I have described as "an effectual jacket for limiting the amount of radiation," is a complete theoretical anticipation and explanation of the "solar crust" of Respighi and the "Trennungschicht" of Zöllner. We agree perfectly in our conclusions, though arriving at them by such very different paths, and so independently of each other.

These are startling conclusions, and when I reached them they were utterly at variance with general astronomical opinion, but I find since their publication that some astronomers have already shown considerable readiness to adopt them. In my case this view of the solar constitution of the larger planets is not a matter of mere opinion, or guessing, or probability, but it follows of necessity, and as stated on page 200, "the great mystery of Saturn's rings is resolved into a simple consequence, a demonstrable and necessary result of the operation of the familiar forces, whose laws of action have been demonstrated here upon this earth by experimental investigation in our laboratories. No strained hypotheses of imaginary forces are required, no ethers or other materials are demanded, beyond those which are beneath our feet and around our heads here upon this earth; all that is necessary is to grant that the well-known elements and compounds of the chemist, and the demonstrated forces of the experimental physicist, exist and operate in the places, and have the quantities and modes of distribution described by the astronomer; this simple postulate admitted, these wondrous appendages spring into rational existence, and like the eternal fires of the sun, the barren surface of the moon, the dry valleys of Mercury, the hazy equivocations of Venus, the seas and continents and polar glaciers of Mars, and the cloud-covered face of Jupiter, follow as necessary consequences of an universal atmosphere."

If I am right in ascribing a gaseous condition to the sun and the larger planets, and tracing the maintenance of this condition to the disturbing gravitation of the attendant planets or satellites, a solution of the riddle of the nebulæ at once presents itself. We have only to suppose a star cluster or group composed of orbs of solar or great planetary dimensions, and that these act mutually upon each other as the planets on our sun, or the satellites upon Saturn, but in a far more violent degree owing to the far greater relative masses of the reacting orbs, and we obtain the conditions under which great gaseous orbs would be not merely pitted on their surface, but riven to their very centres, moulded and shaped throughout by the whirling hurricane of their whole substance. When thus in the centre of a tornado of opposing gravitations the tortured orb would be twisted bodily into a huge vorticose crater, into the bowels of which the aqueous vapour would be dragged and dissociated, and then, entangled with the inner matter of the riven sphere, would be hurled upwards again to burst forth in an explosion of such magnitude that the original body would be measurably presented as a mere appendage, the rocket case of the flood of fire it had vomited forth.

The reader must complete the picture. If he will take a little trouble in doing so he will find that it becomes a portrait of one or the other of the nebulæ, according to the kind of intergravitating star cluster from which he starts. I have endeavoured to work out some of the details of the nebular conditions in the 20th chapter. In chapter 21st I have concluded by showing the analogy between a sun and the hydro-electric machine, the sun being the cylinder and the prominences the steam jets. If issuing jets of high-pressure steam have the same properties at a distance of 91 millions of miles from the earth as upon its surface, the body of the sun and the issuing steam must be in opposite electrical conditions, and furious electrical excitation must result; and if the laws of electrical induction are constant throughout the universe the earth must be as necessarily subject to solar electrical influence as to his thermal radiations. Thus the same reasoning which explains the origin and maintenance of the solar heat and light, the sun spots, the photosphere, the chromosphere, the sierra, the prominences, the zodiacal light, the meteoric zones, the meteorology of the planets, and the rings of Saturn also shows how the electrical disturbances which produce the aurora borealis and direct the needle may originate.

Electrical theories of the corona and zodiacal light, and their connection of some kind with the aurora borealis, have been put forth in many shapes, but as far as I have learned none afford any explanation of the *origin* of the electrical disturbance. Without this they are like the vortices of Descartes, which explained the movements of the planets by supposing another kind of motion still more incomprehensible.

Explanations which are more difficult to explain than the phenomena they propose to elucidate only obscure the light of true science, and stand as impediments to the progress of sound philosophy.

II. MOLECULES, ULTIMATES, ATOMS, AND WAVES.

By Mungo Ponton, F.R.S.E.

(Concluded from p. 359).

Part III.

In all the foregoing estimates respecting the wave-lengths corresponding to the fixed lines of the spectrum, these have been regarded as they exist in the free ether; but when the luminous waves traverse a portion of the ether, in which any transparent substance temporarily subsists, the progress of the wave-motion is, according to the experiments of M. Fizeau, retarded in exact proportion to the refractive index of the substance. Thus, if the refractive index be 2, the speed of transmission will be halved. The returning energy, however—that by which the disturbed particles are restored to their points of rest, will be only a fourth of that which subsists in the free ether—the forces being as the squares of the speeds of transmission. Hence, for any given period of vibration, the excursion of the individual particles will be only a fourth of what they are in the free ether.

The highest ascertained refractive index is that of chromate of lead (2·974); but it is uncertain whether the extremely short waves produced by aluminium electrodes can be transmitted by that substance. There can be no doubt, however, about quartz, seeing it is by using prisms of this mineral that the existence of those very minute waves has been ascertained. The refractive index of quartz is 1·5484, so that the shortest aluminium wave would, while passing through this medium, be reduced to 151,150 billionths of an inch; while the ratio of the length of the wave to the departure of the individual particles from their points of rest would attain the enormous proportion of between 102 and 103 billions to 1. On the supposition that the departure exceeds the radius of the particle, the diameter of an ethereal particle would be to the length of this shortest wave, while passing through quartz, in the ratio of 1 to upwards of 50 billions. To such an extreme degree do these estimates render it probable, that the divisibility of matter is actually carried in the case of the individual particles of the luminiferous ether—a circumstance rendering it less improbable that even the ultimate of hydrogen, the lightest with which we are acquainted, may consist of a considerable number of more minute atoms.

There is another circumstance tending to support this supposition, namely, the peculiar action which certain bodies exert on the ether irrespective of their specific gravity—an action in which hydrogen is pre-eminent over all other substances. To illustrate this point, take the following six media, the refractive indices, and specific gravities of which are as under—that of hydrogen being adopted as the standard of unity.

	Refractive Index.	Sp. gr.
Hydrogen	1·000138	1·000
Oxygen	1·000272	16·000
Atmospheric air	1·000294	14·500
Tabasheer	1·111450	28,449
Water	1·336000	11,798
Chromate of lead . . .	2·974000	70,829

Since light travels about 11,721 inches in the millionth of a second in the free ether, and as its speed is retarded by each medium in proportion to its refractive index, the spaces through which light would pass in the millionth of a second while traversing the above six media would be as under:—

	Inches.		Inches.
Hydrogen . .	11,719·38	Tabasheer . . .	10,545·68
Oxygen . . .	11,717·81	Water	8,773·20
Atmospheric air	11,717·55	Chromate of lead	3,941·16

Hence, to traverse a given space, say a million of inches, light would require, for the several media, the following periods expressed in millionths of a second:—

The free ether . .	85·3170	Tabasheer . . .	94·8255
Hydrogen . . .	85·3287	Water	113·9873
Oxygen	85·3402	Chromate of lead .	253·7326
Atmospheric air .	85·3420		

Thus the retardation of the six media is equivalent to the following additional spaces of the free ether :—

	Inches.		Inches.
For hydrogen . . .	138	For tabasheer . . .	111,450
„ oxygen . . .	272	,, water	336,000
„ atmospheric air .	294	,, chromate of lead	1,974,000

That is to say, the wave motion would pass through the above additional spaces of the free ether during the additional time it would require to traverse a million inches of the above refracting media, or, taking hydrogen as the standard of unity, the following numbers will represent the comparative retarding powers of these substances :—

Hydrogen	1·0000	Tabasheer . . .	812·69
Oxygen	1·9829	Water	2,450·40
Atmospheric air .	2·1367	Chromate of lead .	14,394·00

If, however, we take into account the specific gravities of the substances, the proportions will be very different. Again, making hydrogen the standard of unity, they will stand thus :—

Hydrogen	1·00000	Atmospheric air .	0·147360
Water	0·20770	Oxygen . . .	0·123930
Chromate of lead .	0·20323	Tabasheer . . .	0·028566

Hydrogen and tabasheer thus stand at the head and foot of the list, and, indeed, of all known substances; the power of the former in retarding the progress of the wave-motion being about 35 times that of the latter, in proportion to their respective specific gravities.

It thus appears that the energy with which any substance retards the speed of the wave-motion of light passing through it, depends only to a partial extent on its specific gravity, and in no inconsiderable degree on a certain special power, in which hydrogen excels all other substances. This power must obviously be due to some peculiar relation subsisting between the particles of the ponderable substance and those of the ether in which they are immersed.

The relation between the ether and the ultimates of any permanent gas, like hydrogen, manifests itself to a certain extent in a tendency to separate those ultimates one from another, in opposition to their mutual attraction and their gravitation towards the earth. It is only by a very considerable amount of pressure that the ultimates of hydrogen are kept in that degree of mutual proximity in which they subsist at the earth's surface, when they exhibit that degree of retarding force which is shown in the preceding tables.

The amount of this pressure is equivalent to that of 30 inches of mercury, corresponding to 4,809,528 inches of hydrogen of uniform density. Thus, in order to maintain the ultimates of hydrogen contained in a length of a million inches, in that degree of mutual proximity which enables them to exert on the wave-motion of light a retarding force equivalent to that which would be exerted by only 138 inches of the free ether, requires the gravitating force of nearly five times that quantity of hydrogen. The force which balances this pressure can be nothing but the repulsion of the particles of the ether for the ultimates of hydrogen. In the length of a million inches, the same number of ethereal particles exist in the absence of the hydrogen, as in its presence, the hydrogen ultimates insinuating themselves between the particles of the ether. Their doing so is facilitated by the extreme minuteness of the probable diameter of the ethereal particles, in comparison with the size of the intervals between them. Of the ratio which the former bears to the latter, a fair estimate may be formed from the proportion which the departure of the particles from their points of rest, in the case of the shortest wave, bears to what it is in the case of the longest wave—this proportion being as 1 to 42,000. The effect produced by the hydrogen is the same as would follow the introduction of an additional quantity of 138 inches into the million inches of the ether—equivalent to an addition of 1-7246th part of the number of ethereal particles embraced in the latter space, or to a corresponding diminution in the length of the intervals between them.

Were the ultimates of hydrogen homogeneous simple masses, they might be supposed to be interspersed along the line of ethereal particles at comparatively considerable intervals; while the ethereal particles intervening between each pair of hydrogenous ultimates might be supposed to be forced into greater mutual proximity—so having their mutual distances diminished by 1-7246th part. But according to this view, the compressing force could be only the mutual gravitation of the hydrogenous ultimates and their general gravitation towards the earth, aided by the superincumbent pressure. When it is borne in mind, however, that the mutually repellent force of the ethereal particles is more than a billion of times greater than the force of terrestrial gravitation as it subsists in the lower strata of the atmosphere, it will be perceived how inadequate the latter is to produce the observed amount of compression, and that some other force must be looked to, as that which countervails the repulsive power of the ethereal particles to so great an extent as 1-7246th part of its amount.

The phenomenon might be explained by supposing the existence of some special repulsive energy, as between the ethereal particles and the ultimates of hydrogen, rendering these, in proportion to their specific gravity, conspicuous beyond all others in their power of driving the ethereal particles into closer mutual proximity. An alternative and better explanation, however, is furnished by the supposition that the ultimates of hydrogen are not simple homogeneous masses, but are really compounded of numerous and diverse more minute atoms, held together by a force greatly exceeding that of gravity, or even those of ordinary cohesion and chemical attraction. For to prevent such atoms from passing into absolute contact, and to admit of their being thrown into a state of vibration, the ether separating them must be compressed to such an extent as to render it capable of balancing the force by which the atoms are held in such close mutual proximity. Thus, making a small allowance for the force of gravity, we obtain as a measure of the attractive force by which the atoms constituting the ultimate of hydrogen are held together, about 1-7240th part of the total repulsive energy which the ethereal particles exert one upon another.

Now the observations on the spectrum tend to render the latter explanation of the extraordinary power of hydrogen in compressing the ether the more probable of the two. These show that hydrogen can either generate or interrupt ethereal waves of four different lengths and periods. They are as under—the lengths being stated in billionths of an inch, and the periods in parts of the billionth of a second:—

	Length.	Period.
Wave producing the line C,	25,852,720,	one 453rd.
Wave producing the line F,	19,149,930,	one 612th.
Wave producing a line near G,	17,098,700,	one 685th.
Wave producing a line near H,	16,157,500,	one 725th.

It being extremely improbable that there are four distinct sorts of hydrogenous ultimates, capable of either generating or absorbing those four sorts of vibrations, the most simple, if not the only, explanation of the phenomenon appears to be, that each ultimate consists of four diverse sorts of atoms, to which those four different vibrations are to be attributed.

It would be difficult to imagine any other mode of their generating such vibrations in the ethereal particles, than by their vibrating themselves either at precisely the same rates or at some octave of those rates. Nor could there be easily assigned any other reason for their absorbing or taking up the already existing vibrations of the ethereal particles of those four particular periods, than by their tendency to assume those rates, or some octave of them, and take up the motive energy from the ether to enable them to perform their movements. But in order to their vibrating at all, it is needful to suppose these atoms to be not in absolute mutual contact, but to be kept apart by a layer of compressed ether, the elasticity of which exactly balances their mutual attraction. Of the greatness of this attraction we have two proofs—the one the degree of compression which the ether undergoes under its influence; the second, the great amount of applied motive energy required to set the atoms a vibrating. In this particular these atoms differ from the hydrogenous ultimates themselves, of which they are the constituent parts. For the ultimates can be made to vibrate by a very small amount of applied force; whereas their constituent atoms require the application of an intense force to make them vibrate; while, under ordinary circumstances, they vibrate each kind only at one particular rate.

The foregoing views are strengthened by certain other phenomena which these four spectral lines of hydrogen present. Of the four, that producing the line F is by much the most conspicuous. Those near G and H are comparatively feeble, and require a higher temperature for their development. When strong pressure is applied, it is the line F that first manifests a decided increase in breadth; and it is by the gradual widening of the four lines, but especially of F, that the hydrogen spectrum ultimately becomes, under a very strong pressure, continuous like that from an incandescent body. This phenomenon, which would be nearly inexplicable on the supposition that the ultimates of hydrogen are simple homogeneous masses, receives an easy explanation on the assumption of their consisting of more minute atoms. The greater brightness of the line F would be accounted for by supposing the atoms, whose vibrations generate this wave, to be more numerous than any of the others. Those which generate the wave C would be next in number; then those which generate the wave near G; lastly, those which generate the wave near H. The rapidity with which the two last sets of atoms vibrate would explain their requiring a greater amount of applied energy to set them in motion. The effects of very high pressure would be explained by supposing the pressure to drive the atoms constituting the ultimate into greater mutual proximity than that in which they subsist under ordinary atmospheric pressure. The widening of the lines would result from an adventitious inertia of position developed in the atoms by their being thus forced together—those at the surface of the ultimate being more easily movable, and vibrating at slower rates, and with greater amplitudes, than those in its interior, exactly as in the case of a solid incandescent body, from which the ultimate of hydrogen would differ only in this respect, that, when freed to a certain extent from extraneous pressure, its constituent atoms are at liberty to vibrate in their own peculiar times, without being embarrassed in their movements by each other. The relief from pressure narrows the lines; so that, when the gas becomes much attenuated, the lines acquire an exceeding degree of fineness. The atoms then vibrate more precisely at their own definite rates, unaffected by the proximity of their neighbours. The pre-eminence of hydrogen over other bodies, in its power of compressing the ether and retarding the progress of the wave-motion, in proportion to its specific gravity, would, according to this hypothesis, be due to the higher proportion which the atomic force bears to the gravitating force in hydrogen than in other substances having heavier ultimates.

Thus the whole of the phenomena presented by hydrogen, in its relations to the luminiferous ether, receive an easy explanation from the supposition that its ultimates are not simple and homogeneous masses, but compound bodies consisting of numerous atoms of four diverse kinds—each sort having a different intrinsic inertia of its own, in virtue of which it has a tendency to vibrate at a particular rate. This hypothesis explains not only the four bright lines and the appearances they exhibit, but also the remarkable property of hydrogen in exerting on the ether a compressing power far exceeding in proportion what is due to its specific gravity.

Having thus established the probability of the compound nature of the ultimate of hydrogen—the lightest of all known ultimates, it appears unnecessary to enter minutely into the evidence tending to prove the same fact with respect to the other chemical ultimates. There is, however, one piece of evidence which seems to deserve special notice as bearing on the general question of the compound nature of the ultimates of the chemical elements. It is the fact that, in several instances where these originate waves of several different definite lengths, one or more of those definite waves are common to two or more diverse chemical ultimates. Thus, M. Ångström notes no less than eight waves in different parts of the spectrum as being common to iron and titanium, three common to iron and calcium, three to iron and nickel, 1 to iron and sodium, one to iron and magnesium, one to iron and manganese, one to iron and barium, one to cobalt and titanium, one to titanium and calcium, and one to calcium and manganese. Several other instances of similar coincidences are noticed in a paper by the Rev. T. R. Robinson, "On Spectra of Electric Lights" (Phil. Trans., 1862, p. 939). In particular he notes that the line C, which is one of those produced by hydrogen, is also common to six metals, whatever be the nature of the gas through which the spark between the electrodes passes. He specifies another line in the yellow region, which is, under similar circumstances, common to five metals, and a third in the blue also common to five metals. Mr. Huggins in his tables likewise notes a line near D as being common to arsenic and zinc, the longer D itself as being common to sodium and lead, the shorter D to sodium and barium. He has also found a line nearly midway between D and E common to tellurium and nitrogen, one a little beyond E common to oxygen and arsenic, and one a little beyond F common to nitrogen and chromium.

Of these phenomena, as well as of the multiplicity of lines shown by some of the metals, the supposition that the chemical elements are compounded of more minute atoms supplies a simple explanation. As the lines are supposed to be produced by the vibrations, not of the ultimates themselves, but of their constituent atoms, and to depend on the intrinsic inertia of these latter, when the same line is found to be common to two or more diverse metals or other elements, the most obvious inference is that the ultimates of those metals or other elements contain the same sort of atoms in combination with others of diverse kinds. Thus the same species of atom which, in the ultimate of hydrogen, produces the line C, occurs also in the ultimates of six metals, associated with a variety of other atoms having different periods of vibration. The circumstance also noted by the observers above named, that the same line will appear with diverse degrees of relative brightness in the spectra of the different elements in which it occurs, is also easily explained by supposing that the atom, whose vibrations originate that line, occurs in greater or smaller proportion relatively to the others with which it is associated in the ultimates of those different elements. There are thus none of the phenomena presented by the spectrum which cannot be explained by this hypothesis, while all the phenomena concur to give it support.

There is one peculiar case that demands separate notice. It is that of the rare metals erbium and didymium, the spectra from which, when they are rendered incandescent in the solid state, exhibit bright lines similar to those presented by the spectra from incandescent vapours and gases. The only feasible explanation of this phenomenon appears to be that a portion of the metals is thrown into the state of a very dense vapour, which clings to the solid, and that it is in this vapour that the bright lines are generated.* This phenomenon in no degree militates against the supposition of the compound nature of the chemical elements—rather the reverse.

Marvellous as are the facts and phenomena which have been here brought under review, our wonder is perhaps still more highly excited, when we contemplate the adaptation of the eye and the optic nerve to receive sensible impressions from the minute motions of the ethereal particles. For it must be borne in mind, that it is not the wave-motion (which merely serves to carry forward the transverse movement from point to point), but the inconceivably minute transverse motions performed by the ethereal particles, which are themselves really effective in producing vision, heat, actinism, and fluorescence. The extreme smallness of those movements altogether baffles our powers of conception; yet they manifest to us their existence in a very palpable manner. In the case of vision the particles of the optic nerve must vibrate synchronously with those of the ether, and must perform the same minute movements. This very minuteness, however, explains how it is that luminous waves, coming from a large field of view, can find simultaneous entrance into so small a circle as the pupil of the eye, and how the optic nerve can be simultaneously impressed by so great a multitude and diversity of vibrations.

There is a phenomenon, however, which is perhaps still more curious, and of which the author of this paper can speak from personal experience. It is that of the optic nerve being excited into action by purely internal causes, in the absence of all external stimulus from the ethereal waves. This phenomenon presents two varieties. In the one, when the eyes are opened after sleep in a perfectly dark chamber, there is perceived a general brightness, and occasionally spectral images of various kinds are formed in this apparently phosphorescent haze. In the other variety, the eye is itself phosphorescent, and near objects are seen by the light thus strangely generated within the organ. The author has had numerous opportunities of observing the former phenomenon, and several of noticing the latter, which in like manner manifests itself when the eyes are first opened after sleep. White or light-coloured objects are then clearly perceived as if illuminated by Canton's phosphorus, only brighter. The illumination resembles that thrown upon an object by a lens or bull's-eye, exhibiting a bright central disc, from which it gradually fades away. On closing the eyes for a short time and re-opening them, the brightness is found to have vanished, and to be replaced by absolute darkness, both in the first and second varieties of the phenomenon.

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* The probability of this explanation will be appreciated by those who remember the experiments of M. Waidelé on the adhesion of gases and vapours to smooth surfaces, by which he so successfully explained the curious images obtained in the dark by M. Moser.

It appears not improbable that the eyes of some nocturnal animals may have this same phosphorescent property in a higher degree, and that they may be thus enabled to find and follow their prey by means of light proceeding from their own eyes. No stronger proof than this could be adduced to confirm the conclusion already established by so many other phenomena, that light consists of the vibrations of an ethereal medium. For we have here similar vibrations excited in the eye itself from an internal cause, and these, reacting on the external ether, excite synchronous vibrations which are propagated in waves, first from the eye to the outward object, and thence back from the object to the eye. The phosphorescence of the eye in man is believed to be very rare; but the author has been assured that other individuals besides himself have observed the same phenomenon.

III. SOME FURTHER EXPERIMENTS ON PSYCHIC FORCE.

By William Crookes, F.R.S, &c.

> "I am attacked by two very opposite sects,—the scientists and the know-nothings. Both laugh at me—calling me 'the frogs' dancing master.' Yet I know that I have discovered one of the greatest forces in nature."—Galvani.

It was my intention to have allowed a longer time to elapse before again writing on the subject of "Psychic Force" in this journal. My reason for this resolve was not so much owing to want of new matter and fresh results,—on the contrary, I have much that is new in the way of experimental evidence in support of my previous conclusions,—but I felt some reluctance to impose on the readers of the "Quarterly Journal of Science" a subject which they might view with little favour. When the editor of a scientific journal is also an experimental investigator, or a student of any special branch of knowledge, there is a natural tendency on his part to unduly exalt the importance of that which is occupying his thoughts at the time; and thus the journal which he conducts is in danger of losing breadth of basis, of becoming the advocate of certain opinions, or of being coloured by special modes of thought.

The manner in which the experimental investigation described in the last "Quarterly Journal" has been received, removes any doubt I might entertain on this score. The very numerous communications which have been addressed to the office of this journal show that another paper on the same subject will not be distasteful to a large number of those who did me the honour to read my former article; whilst it appears to be generally assumed that I should take an early opportunity to reply to some of the criticisms provoked by the remarkable character of the experimental results which I described.

Many of the objections made to my former experiments are answered by the series about to be related. Most of the criticisms to which I have been subjected have been perfectly fair and courteous, and these I shall endeavour to meet in the fullest possible manner. Some critics, however, have fallen into the error of regarding me as an advocate for certain *opinions*, which they choose to ascribe to me, though in truth my single purpose has been to state fairly and to offer no opinion. Having evolved men of straw from their own imagination, they proceed vigorously to slay them, under the impression that they are annihilating me. Others, —and I am glad to say they are very few,—have gone so far as to question my veracity:—"Mr. Crookes must get better witnesses before he can be believed!" Accustomed as I am to have my word believed without witnesses, this is an argument which I cannot condescend to answer. All who know me and read my articles will, I hope, take it for granted that the *facts* I lay before them are correct, and that the experiments were honestly performed, with the single object of eliciting *the truth*.

It is edifying to compare some of the present criticisms with those that were written twelve months ago. When I first stated in this journal that I was about to investigate the phenomena of so-called spiritualism, the announcement called forth universal expressions of approval. One said that my "statements deserved respectful consideration;" another, expressed "profound satisfaction that the subject was about to be investigated by a man so thoroughly qualified as," &c.; a third was "gratified to learn that the matter is now receiving the attention of cool and clear-headed men of recognised position in science;" a fourth asserted that "no one could doubt Mr. Crookes's ability to conduct the investigation with rigid philosophical impartiality;" and a fifth was good enough to tell its readers that "if men like Mr. Crookes grapple with the subject, taking nothing for granted until it is proved, we shall soon know how much to believe."

These remarks, however, were written too hastily. It was taken for granted by the writers that the results of my experiments would be in accordance with their preconceptions. What they really desired was not *the truth*, but an additional witness in favour of their own foregone conclusion. When they found that the facts which that investigation established could not be made to fit those opinions, why,—"so much the worse for the facts." They try to creep out of their own confident recommendations of the enquiry by declaring that "Mr. Home is a clever conjuror, who has duped us all." "Mr. Crookes might, with equal propriety, examine the performances of an Indian juggler." "Mr. Crookes must get better witnesses before he can be believed." "The thing is too absurd to be treated seriously." "It is impossible, and therefore can't be."* "The observers have all been biologised (!) and fancy they saw things occur which really never took place," &c., &c.

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* The quotation occurs to me—"I never said it was possible, I only said it was true."

These remarks imply a curious oblivion of the very functions which the scientific enquirer has to fulfil. I am scarcely surprised when the objectors say that I have been deceived merely because they are unconvinced without personal investigation, since the same unscientific course of *à priori* argument has been opposed to all great discoveries. When I am told that what I describe cannot be explained in accordance with preconceived ideas of the laws of nature, the objector really begs the very question at issue and resorts to a mode of reasoning which brings science to a standstill. The argument runs in a vicious circle: we must not assert a fact till we know that it is in accordance with the laws of nature, while our only knowledge of the laws of nature must be based on an extensive observation of facts. If a new fact seems to oppose what is called a law of nature, it does not prove the asserted fact to be false, but only that we have not yet ascertained all the laws of nature, or not learned them correctly.

In his opening address before the British Association at Edinburgh this year, Sir William Thomson said, "Science is bound by the everlasting law of honour to face fearlessly every problem which can fairly be presented to it." My object in thus placing on record the results of a very remarkable series of experiments is to present such a problem, which, according to Sir W. Thomson, "Science is bound by the everlasting law of honour to face fearlessly." It will not do merely to deny its existence, or try to sneer it down. Remember, I hazard no hypothesis or theory whatever; I merely vouch for certain facts, my only object being—the *truth*. Doubt, but do not deny; point out, by the severest criticism, what are considered fallacies in my experimental tests, and suggest more conclusive trials; but do not let us hastily call our senses lying witnesses merely because they testify against preconceptions. I say to my critics, Try the experiments; investigate with care and patience as I have done. If, having examined, you discover imposture or delusion, proclaim it and say how it was done. But, if you find it be a fact, avow it fearlessly, as "by the everlasting law of honour" you are bound to do.

I may at once answer one objection which has been made in several quarters, viz., that my results would carry more weight had they been tried a greater number of times, and with other persons besides Mr. Home. The fact is, I have been working at the subject for two years, and have found nine or ten different persons who possess psychic power in more or less degree; but its development in Mr. D. D. Home is so powerful, that, having satisfied myself by careful experiments that the phenomena observed were genuine, I have, merely as a matter of convenience, carried on my experiments with him, in preference to working with others in whom the power existed in a less striking degree. Most of the experiments I am about to describe, however, have been tried with another person other than Mr. Home, and in his absence.

Before proceeding to relate my new experiments, I desire to say a few words respecting those already described. The objection has been raised that announcements of such magnitude should not be made on the strength of one or two experiments hastily performed. I reply that the conclusions were not arrived at hastily, nor on the results of two or three experiments only. In my former paper ("Quarterly Journal of Science," page 340), I remarked:—"Not until I had witnessed these facts some half-dozen times, and scrutinised them with all the critical acumen I possess, did I become convinced of their objective reality." Before fitting up special apparatus for these experiments, I had seen, on five separate occasions, objects, varying in weight from 25 to 100 lbs., temporarily influenced in such a manner, that I, and others present, could with difficulty lift them from the floor. Wishing to ascertain whether this was a physical fact, or merely due to a variation in the power of our own strength under the influence of imagination, I tested with a weighing machine the phenomenon on two subsequent occasions when I had an opportunity of meeting Mr. Home at the house of a friend. On the first occasion, the increase of weight was from 8 lbs. normally, to 36 lbs., 48 lbs., and 46 lbs. in three successive experiments tried under strict scrutiny. On the second occasion, tried about a fortnight after, in the presence of other observers, I found the increase of weight to be from 8 lbs. to 23 lbs., 43 lbs., and 27 lbs., in three successive trials, varying the conditions. As I had the entire management of the above-mentioned experimental trials, employed an instrument of great accuracy, and took every care to exclude the possibility of the results being influenced by trickery, I was not unprepared for a satisfactory result when the fact was properly tested in my own laboratory. The meeting on the occasion formerly described was, therefore, for the purpose of confirming my previous observations by the application of crucial tests, with carefully arranged apparatus of a still more delicate nature.

That this is a legitimate subject for scientific inquiry scarcely needs assertion. Faraday himself did not consider it beneath his dignity to examine similar phenomena; and, in a letter to Sir Emerson Tennent, written in 1861 on the occasion of a proposed experimental inquiry into the phenomena occurring in Mr. Home's presence, he wrote:—"Is he (Mr. Home) willing to investigate as a philosopher, and, as such, to have no concealments, no darkness, to be open in communication, and to aid inquiry all that he can? . . . . Does he consider the effects natural or supernatural? If they be the glimpses of natural action not yet reduced to law, ought it not to be the duty of every-one who has the least influence in such actions personally to develope them, and to aid others in their development, by the utmost openness and assistance, and by the application of every critical method, either mental or experimental, which the mind of man can devise?"

If circumstances had not prevented Faraday from meeting Mr. Home, I have no doubt he would have witnessed phenomena similar to those I am about to describe, and he could not have failed to see that they offered "glimpses of natural action not yet reduced to law."

I have already alluded to the publication of the ill-success encountered by the members of the St. Petersburg Committee. Had the results been satisfactory, it must be fairly assumed that the members would have been equally ready to publish a report of their success.

I am informed by my friend Professor Boutlerow,* that during the last winter, he tried almost the same experiments as those here detailed, and with still more striking results. The normal tension on the dynamometer being 100 lbs., it was increased to about 150 lbs., Mr. Home's hands being placed in contact with the apparatus in such a manner that any exertion of power on his part would diminish, instead of increase, the tension.

In 1854, Count Agénor de Gasparin published a book,† giving full details of a large series of physical experiments which he had tried with some private friends in whom this force was found to be strongly developed. His experiments were very numerous and were carried on under the strictest test conditions. The fact of motion of heavy bodies without mechanical contact was demonstrated over and over again. Careful experiments were made to measure the force both of gravitation and of levitation thus communicated to the substances under trial, and an ingenious plan was adopted by which Count de Gasparin was enabled to obtain a rough numerical estimate of the power of the psychic force in each individual. The author finally arrived at the conclusion that all these phenomena are to be accounted for by the action of natural causes, and do not require the supposition of miracles nor the intervention of spirits or diabolical influences. He considers it as a fact fully established by his experiments, that the will, in certain states of the organism, can act at a distance on inert matter, and most of his work is devoted to ascertaining the laws and conditions under which this action manifests itself.

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* Professor of Chemistry at the University of St. Petersburg; author of a work on Chemistry, entitled "Lehrbuch der Organischen Chemie;" Leipzig, 1868.

† Science *versus* Spiritualism. Paris, 1854. New York, 1857.

In 1855, M. Thury, a Professor at the Academy of Geneva, published a work,* in which he passed in review Count de Gasparin's experiments, and entered into full details of researches he had been simultaneously carrying on. Here, also, the trials were made with private friends, and were conducted with all the care which a scientific man could bring to bear on the subject. Space will not allow me to quote the valuable numerical results obtained by M. Thury, but from the following headings of some of his chapters, it will be seen that the enquiry was not conducted superficially:—Facts which Establish the Reality of the New Phenomenon; Mechanical Action rendered Impossible; Movements effected without Contact; The Causes; Conditions requisite for the Production and Action of the Force; Conditions for the Action with Respect to the Operators; The Will; Is a Plurality of Operators Necessary? Preliminary Requisites; Mental Condition of the Operators; Meteorological Conditions; Conditions with Respect to the Instruments Operated upon; Conditions relative to the Mode of Action of the Operators on the Instruments; Action of Substances interposed; Production and Transmission of the Force; Examination of the Assigned Causes; Fraud; Unconscious Muscular Action produced in a particular Nervous State; Electricity; Nervo-magnetism; M. de Gasparin's Theory of a Special Fluid; General Question as to the Action of Mind on Matter. 1st Proposition: In the ordinary conditions of the body the will only acts directly within the sphere of the organism. 2nd Proposition: Within the organism itself there are a series of mediate acts. 3rd Proposition: The substance on which the mind acts directly—the *psychode*—is only susceptible of very simple modification under the influence of the mind; Explanations which are based on the Intervention of Spirits. M. Thury refutes all these explanations, and considers the effects due to a peculiar substance, fluid, or agent, pervading, in a manner similar to the luminiferous ether of the scientist, all matter, nervous, organic, or inorganic—which he terms *psychode*. He enters into full discussion as to the properties of this state or form of matter, and proposes the term *ectenic force* (ἐκτένια, extension), for the power exerted when the mind acts at a distance through the influence of the psychode.*

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* Geneva; Librairie Allemande de J. Kessmann. 1855.

There is likewise another case on record in which similar test experiments were tried, with like results, by a thoroughly competent observer. The late Dr. Robert Hare, in one of his works,† gives an engraving of an apparatus very similar to my own, by which the young man with whom he was experimenting was prevented from having any other communication with the apparatus except through water; yet, under these circumstances, the spring balance indicated the exertion of a force equal to 18 lbs. The details of this experiment were communicated by Dr. Hare to the American Association for the Advancement of Science, at the meeting in August, 1855.

The references I now give afford an answer to the statement that these results must be verified by others. They have been verified over and over again. Indeed, my own experiments may be regarded merely as verifications of results already obtained and published by eminent scientific men in this and other countries.‡

But I was not content with this. I felt that having the opportunity of showing these phenomena to others, I might at a future time be blamed did I not, once for all, take the very best means of bringing them before the notice of the scientific world. Accordingly I forwarded an account of my experiments to the Royal Society on June 15, 1871, and addressed myself to the two secretaries of the Royal Society, Professor Sharpey and Professor Stokes, inviting them to my house to meet Mr. Home, at the same time asking them to be prepared for negative results, and to come a second, or, if necessary, a third time, before forming a judgment.

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* Professor Thury's ectenic and my psychic force are evidently equivalent terms. Had I seen his work three months ago I should have adopted his term. The suggestion of a similar hypothetical nervous fluid has now reached us from another and totally different source, expounded with distinct views, and couched in the language of one of the most important professions—I allude to the theory of a nervous atmosphere advanced by Dr. Benjamin W. Richardson, M.D., F.R.S., in the "Medical Times," No. 1088, May 6, 1871.

† "Experimental Investigation;" By Robert Hare, M.D., Emeritus Professor of Chemistry in the University of Pennsylvania, &c. New York: Partridge and Britton, 1858.

‡ The Report of the Dialectical Society on Spiritualism will appear in a few days, and it will be seen that the Investigation Committee, though commencing their experiments with the entire conviction that they should expose an imposture, have ended by affirming that they are convinced of the existence of a force emanating from the human organisation, by which motion may be imparted to heavy substances, and audible sounds made on solid bodies without muscular contact; they also state that this force is often directed by some intelligence.

Dr. Sharpey politely declined the invitation.

Professor Stokes replied that he thought there was a fallacy in my apparatus, and concluded by saying—

"The facts you mentioned in the paper were certainly at first sight very strange, but still possible modes of explanation occurred to me which were not precluded by what I read in the paper. If I have time when I go to London I will endeavour to call at your house. I don't want to meet anyone; my object being to scrutinise the apparatus, not to witness the effects."

To this I replied on June 20th; the following extracts are taken from my answer:—

"I am now fitting up apparatus in which contact is made through water only, in such a way that transmission of mechanical movement to the board is impossible; and I am also arranging an experiment in which Mr. Home will not touch the apparatus at all. This will only work when the power is very strong; but last night I tried an experiment of this kind, and obtained a considerable increase of tension on the spring balance when Mr. Home's hands were three inches off. With him the power is so great that I can work with large and crude materials, and measure the force in pounds. But I propose to make a delicate apparatus, with a mirror and reflected ray of light, to show fractions of grains. Then I hope to find this force is not confined to a few, but is, like the magnetic state, universal. The subject shall have a 'most scrupulously searching physical scrutiny,' and whatever results I obtain shall be published. I consider it my duty to send first to the Royal Society, for by so doing I deliberately stake my reputation on the truth of what I send. But will the Society (or the Committee*) accept my facts as facts, or will they require vouchers for my integrity? If my statements of fact are taken as correct, and only my interpretation or arrangements of apparatus objected to, then it would seem to be right to give me an opportunity of answering these objections before finally deciding. The other supposition—that my facts are incorrect—I cannot admit the discussion of till I am definitely assured that such is entertained.

"Mr. Home is coming here on Wednesday and Friday evenings; if you can come on either or both occasions at 8 p.m., I shall be glad to see you, or if you only wish to scrutinise the apparatus, I will be here at any time you like to name."

On the 28th of June another paper was sent to the Royal Society. Two days after, Professor Stokes wrote a letter, from which I quote:—

"As I was otherwise engaged so as not to be able conveniently to go to your house, I may as well mention the possible sources of error which occurred to me with reference to your first apparatus. I don't suppose they all exist;

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* Alluding to a rumoured rejection of my paper by the Committee of the Royal Society.

but it is evidently, as you yourself would freely admit, for the *assertor* of a new force to remove all sources of reasonable objection.

"The breadth of the foot of the board was, I think, 1½ or 2 inches, and the bell placed on it was, perhaps, 2 or 3 inches broad. (I can't carry the exact figures in my head.) Join the left edge* of the top of the bell, *a*, with the right hand edge, *b*, of the base of the bell, and let *e f* be the joining line. Then we may suppose the fingers to have pressed in any direction short of the limiting line *e f*. Also as the board was rigid, the fulcrum for aught we know may have been at *c*. From *c* let fall a perpendicular *c m* on the line, *e f*. Then the pressure of the finger may have acted at the distance, *c m*, from the fulcrum. Also, as the base lay flat on the table and both were rigid, for aught we know an infinitesimal, and therefore imperceptible, tilt communicated to the table at the time of trying the experiment may have shifted the fulcrum from the edge *d* to the edge *c*, so that the weight of the hand may have acted by an arm longer than before by *c d*, which would have contributed to the result.

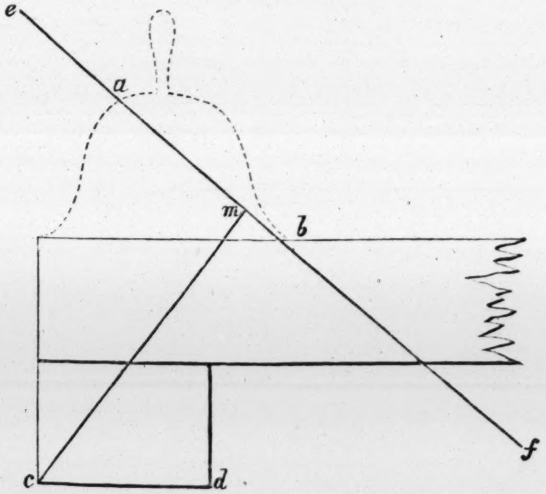
"In your second paper the uncertainty as to the broad bearing is removed. But when the hand was dipped into the water the pressure on the base of the glass vessel (after a little time if the connecting hole be narrow) is increased by the weight of the water displaced, and that would of course depress the balance.

"I don't think much of mere tremors, for it would require very elaborate appliances to prove that they were not due to a passing train or omnibus or to a tremor in the body of one of the company. . . . .
What do you wish to be done with the papers?"

To this I replied as follows, on July 1st:—

"In your letter of the 30th ult., just received, you are quite right in saying that I would freely admit that 'the *assertor* of a new force should remove all sources of reasonable objection.' In your previous letter of the 19th June, you write with equal fairness, that 'your opinion is that you (the R. S.) ought not to refuse to admit evidence of the existence of a hitherto unsuspected force; but that before printing anything on such a subject, you ought to require a most scrupulously searching physical scrutiny of the evidence adduced in favour of the existence of such a force.'

FIG. 1 (half-scale).



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* The diagram referred to here is shown, drawn to scale, in my answer further on. The experiment under discussion is the one figured and described in the last number of the "Quarterly Journal of Science," page 345.

"You have now been good enough to explain to me in detail what the fallacy is which you think exists in my first experiments, and what you consider to be the possible sources of error in my subsequent trials.

"On re-drawing the diagram you give in your letter, Fig. 1, to the full size, supplying the deficient data, viz., the position of the shoulder, *a*, and the point, *b*, your line *c m* appears to be about 2·9 inches long; and, as you assume that the fulcrum shall be at *c*, the lever becomes one of the third order, the two forces acting respectively at $p = 2{\cdot}9$ inches, and at $q = 36$ inches from *c*. What power, P, must be exerted at *p* to overcome a resistance or weight, Q, of 6 lbs. at the end of the lever, *q*?

$$Pp = Qq.$$

$$\text{Hence, } P \times 2{\cdot}9 = Q \times 36.$$

$$\therefore P = 74{\cdot}5 \text{ lbs.}$$

Therefore, it would have required a force of 74·5 lbs. to have been exerted by Mr. Home to have produced the results, even if all your suppositions are granted; and, considering that he was sitting in a low easy chair, and four pairs of sharp, suspicious eyes were watching to see that he exerted no force at all, but kept the tips of his fingers lightly on the instrument, it is sufficiently evident that an exertion of this pressure was impossible. A few pounds vertical pressure was all he could have effected.

"Again, you are not justified in assuming that the fulcrum was at *c*. Granting that 'an infinitesimal and therefore imperceptible tilt' might, at the very first movement, have thrown it from *d* to *c*, it is evident that the movement would at once throw it forward again from *c* to *d*. To have failed to have done so, the tilt must have been so obvious as to have been detected at once.

"But, as I said in my last paper, I prefer to appeal to new experiments rather than argue about old ones, and hence my employment of the water for transmitting the force. The depth of water in the copper hemisphere was only 1½ inches, whilst the glass vessel was 9 inches in diameter.* I have just tried the experiment of immersing my hand to the very utmost in the copper vessel (Mr. Home only dipped in the tips of his fingers) and the rise of the level of the water is not sufficient to produce any movement whatever on the index of the balance, the friction of the apparatus being enough to absorb the ounce or two thus added to the weight. In my more delicate apparatus, this increase of hydrostatic pressure produces a decided movement of the spot of light, but this difficulty I shall overcome by placing the water vessel over the fulcrum, or on the short side of it.

"You say 'you don't think much of mere tremors,' as if in the other experiments described in my second paper the movements of the apparatus were only of this kind. This is not the case; the quivering of the apparatus always took place before the index moved, and the upward and downward motion of the board and index was of a very slow and deliberate character, occupying several seconds for each rise and fall; a tremor produced by passing vehicles is a very different thing from a steady vertical pull of from 4 to 8 lbs., lasting for several seconds.

"You say the session is now over, and ask what I wish to be done with the papers.

"Three years ago (June 27th, 1868), I sent a paper to the Society, 'On the Measurement of the Luminous Intensity of Light,' just after the session closed. It was not read till December 17th. My *wish* would be for a similar course to be adopted in the present instance, although I am scarcely sanguine enough to expect that so much notice will be taken of these communications. So many scientific men are now examining into these strange phenomena (including many Fellows of the Society), that it cannot be many years before the subject will be brought before the scientific world in a way that will enforce attention. I confess that, in sending in these papers to the Society, I have been actuated more by the desire of being the first scientific experimenter who has ventured to take such a course, than by any particular desire that they should meet with immediate attention. I owe to the Society the first intimation of important scientific results, and these I shall continue to send, '*pour prendre date*,' if for no other reason."

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* For a description of this apparatus, see p. 484.

"The Spectator" of July 22nd contained an editorial note, in which it is asserted that my paper was declined by the committee:—

> "The Royal Society, they say, was quite open to communications advocating the existence of a force in nature as yet unknown, if such communications contained scientific evidence adequate to establish its probability; but that, looking to the inherent improbability of the case as stated by Mr. Crookes, and the *entire want of scientific precision* in the evidence adduced by him, the paper was not regarded as one deserving the attention of the Royal Society."

This paragraph not only states that my papers were declined, but proceeds to state the grounds of their rejection. The fact is, that a quorum of the committee of papers not having been present, the question was deferred to the next session in November, and on inquiry at Burlington House, I am informed by the Assistant-Secretary of the Royal Society that my papers, with others, are still awaiting the decision of the committee. Consequently the statement of a rejection was not only premature, but purely imaginary.

It appears, however, that there were some grounds for this statement, for in "The Spectator" of July 29th, 1871, the editor replies as follows:—

> "Our note was not founded on any mere rumour. The words we used contained an exact copy of the words conveyed to us as used, not, as we inadvertently stated by the committee, but by one of the secretaries, Professor Stokes, who in the absence of a quorum, exercised *pro tempore* the usual discretionary authority in regard to papers offered."

I am unable to explain how it is that Professor Stokes's statements to me and to the editor of "The Spectator" bear so different an interpretation, or why a weekly newspaper was chosen for first conveying to me a decision of the committee of papers of the Royal Society.

At the urgent request of gentlemen on the committee of section A, I communicated a paper consisting of about sixteen closely-written pages to the British Association, in which I recounted some of the experiments described in the present paper. Section A referred the paper to a committee to decide whether it should be read. Professor Stokes afterwards handed to me the following document:—

> "*Report on Mr. Crookes's Paper.*
>
> "August 7, 1871.
>
> "The paper having been placed in my hands about ten o'clock, and a decision wanted in writing by a quarter to eleven, I have been obliged to be hasty.
>
> "The subject *seems to be investigated in a philosophical spirit*, and I do not see the explanation of the result of the first class of experiments, while at

the same time I am not prepared to give in my adhesion without a thorough sifting by more individuals than one. I don't see much use discussing the thing in the sections, crowded as we already are: but if a small number of persons in whom the public would feel confidence choose to volunteer to act as members of a committee for investigating the subject, I don't see any objection to appointing such committee. I have heard too much of the tricks of Spiritualists to make me willing to give my time to such a committee myself.

"G. G. STOKES."

Whilst I cannot but regret that a physicist of such eminence as Professor Stokes should "be hasty," in deciding on the merits of a paper which it is physically impossible he could have even once read through, I am glad to find that he no longer continues to speak of the "entire want of scientific precision in the evidence adduced" by me, but rather admits that "the subject seems to be investigated in a philosophical spirit."

In submitting these experiments, it will not seem strange that I should consider them final until rebutted by arguments also drawn from facts, and that I should seek to know on what grounds contra-statements are founded. Professor Allen Thomson, at the recent meeting of the British Association, remarked that no course of inquiry into the matter before us "can deserve the name of study or investigation." And why not? On the other hand, Professor Challis, of Cambridge, writes, "In short, the testimony has been so abundant and consentaneous, that either the facts must be admitted to be such as are reported, or the possibility of certifying facts by human testimony must be given up." It is certainly not too much to suppose that Dr. Thomson had some grounds for his statement; and, indeed, "I have," he owns, "been fully convinced of this (the fallacies of spiritualistic demonstration) by repeated examinations;" but where are the results of his investigations to be found? They must be very conclusive to warrant him in the use of such expressions as "a few men of acknowledged reputation in some departments of science have surrendered their judgments to these foolish dreams, *otherwise* appearing to be within the bounds of sanity." If Dr. Thomson's dogmatic denial arises from the mere strangeness of the facts I have set forth, what can he think of the address of the President for this year. Surely the conception of a nerve-force is no more difficult than that "of the inner mechanism of the atom;" and again, any investigation, be it worthy the name or not, bearing on a matter in which eminent men have avowed their belief, which takes a leading rank among the social questions of the day, and which numbers its adherents by millions, is surely as full of merit, and as instructive to all, as hypothetical inquiries into "interatomic atmospheres" and "gyrating interatomic atoms." Professor Huxley has observed, "If there is one thing clear about the progress of modern science, it is the tendency to reduce all scientific problems, except those that are purely mathematical, to problems in molecular physics—that is to say, to attractions, repulsions, motions, and co-ordination of the ultimate particles of matter! Yet these ultimate particles, molecules, or atoms, are creatures of the imagination, and as pure assumptions as the spirits of the spiritualist." But perhaps Dr. Allen Thomson's respect for mathematics is so great that he is blind to actuality. It does not speak well for modern scientific philosophy that, after the startling revelations of the spectroscope during the last decade, investigations should be scouted because they pertain to an ulterior state of things of which at present we have little idea. That I have furnished no dynamic equivalent of psychic force, or given no formulæ for the variable intensity of Mr. Home's power, is certainly no argument whatever against the existence of such a force. Men thought before the syllogism was invented, and, strange as it may seem to some minds, force existed before its demonstration in mathematical formulæ.

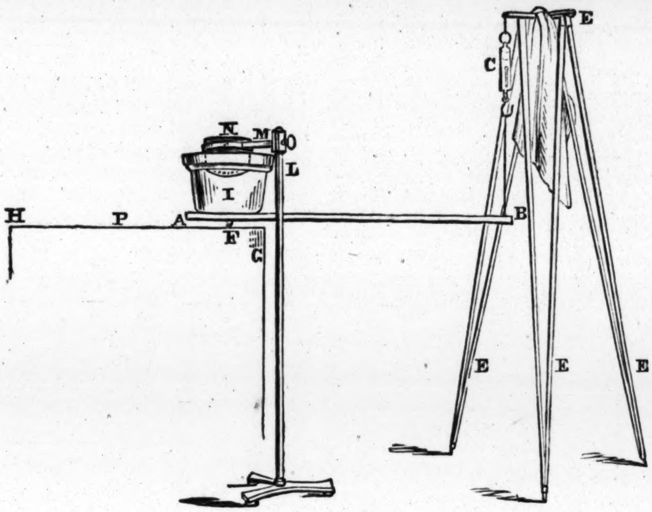
As an answer to Professor Balfour Stewart's rather bold conjecture, that Mr. Home possesses great electro-biological power (whatever that may mean), by which he influences those present, I point to the curves illustrating this paper; however susceptible the *persons* in the room might have been to that assumed influence, it will hardly be contended that Mr. Home biologised the recording instruments.

I will not occupy more time with personal matters, or with explanations forced from me in self-defence against uncourteous commentaries based on unjust misrepresentations; but I will proceed to describe the experiments, most of which, I may remark, might have been witnessed by Professor Stokes and Professor Sharpey, had they accepted the invitations I gave them.

On trying these experiments for the first time, I thought that actual contact between Mr. Home's hands and the suspended body whose weight was to be altered was essential to the exhibition of the force; but I found afterwards that this was not a necessary condition, and I therefore arranged my apparatus in the following manner:—

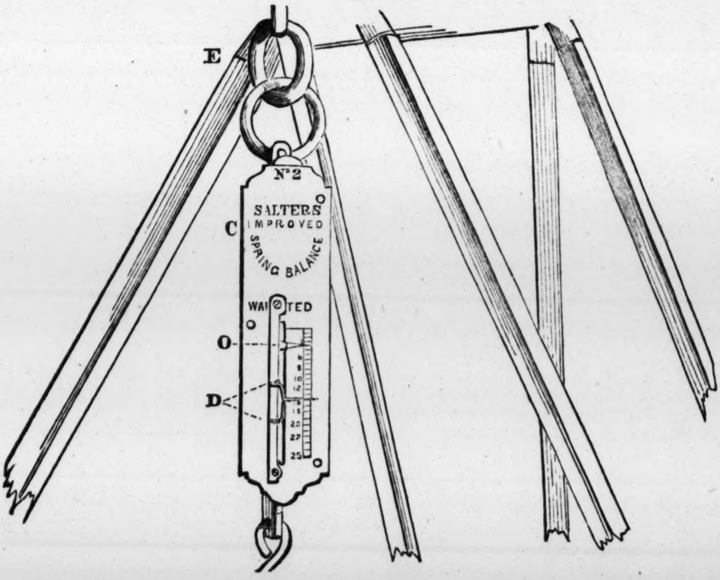
The accompanying cuts (Figs. 2, 3, 4) explain the arrangement. Fig. 2 is a general view, and Figs. 3 and 4 show the essential parts more in detail. The reference letters are the same in each illustration. A B is a mahogany board, 36 inches long by 9½ inches wide, and 1 inch thick. It is suspended at the end, B, by a spring balance, C, furnished with an automatic register, D. The balance is suspended from a very firm tripod support, E.

FIG. 2.



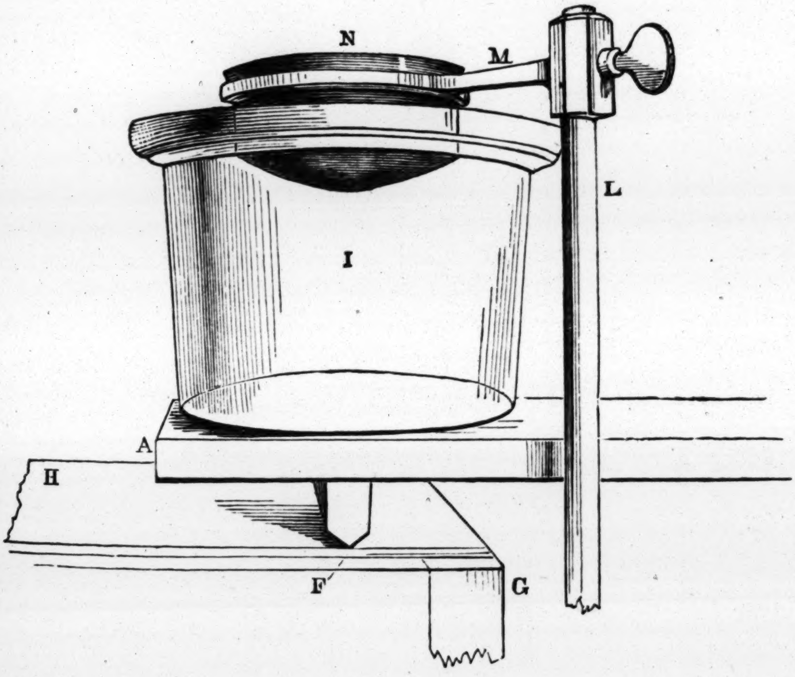
The following piece of apparatus is not shown in the figures. To the moving index, O, of the spring balance, a fine steel point is soldered, projecting horizontally outwards. In front of the balance, and firmly fastened to it, is a

FIG. 3.



grooved frame carrying a flat box similar to the dark box of a photographic camera. This box is made to travel by clock-work horizontally in front of the moving index, and it contains a sheet of plate-glass which has been smoked over a flame. The projecting steel point impresses a mark on this smoked surface. If the balance is at rest, and the clock set going, the result is a perfectly straight horizontal line. If the clock is stopped and weights are placed on the end B of the board, the result is a vertical line, whose length depends on the weight applied. If, whilst the clock draws the plate along, the weight of the board (or the tension on the balance) varies, the result is a curved line, from which the tension in grains at any moment during the continuance of the experiments can be calculated.

FIG. 4.



The instrument was capable of registering a diminution of the force of gravitation as well as an increase; registrations of such a diminution were frequently obtained. To avoid complication, however, I will only here refer to results in which an increase of gravitation was experienced.

The end B of the board being supported by the spring balance, the end A is supported on a wooden strip, F, screwed across its lower side and cut to a knife edge (see Fig. 4). This fulcrum rests on a firm and heavy wooden stand, G H. On the board, exactly over* the fulcrum, is placed a large glass vessel filled with water, I. L is a massive iron stand, furnished with an arm and a ring, M N, in which rests a hemispherical copper vessel perforated with several holes at the bottom.

The iron stand is 2 inches from the board A B, and the arm and copper vessel, M N, are so adjusted that the latter dips into the water 1½ inches, being 5½ inches from the bottom of I, and 2 inches from its circumference. Shaking or striking the arm M, or the vessel N, produces no appreciable mechanical effect on the board, A B, capable of affecting the balance. Dipping the hand to the fullest extent into the water in N does not produce the least appreciable action on the balance.

As the mechanical transmission of power is by this means entirely cut off between the copper vessel and the board A B, the power of muscular control is thereby completely eliminated.

For convenience I will divide the experiments into groups 1, 2, 3, &c., and I have selected one special instance in each to describe in detail. Nothing, however, is mentioned which has not been repeated more than once, and in some cases verified, in Mr. Home's absence, with another person possessing similar powers.

There was always ample light in the room where the experiments were conducted (my own dining-room) to see all that took place.

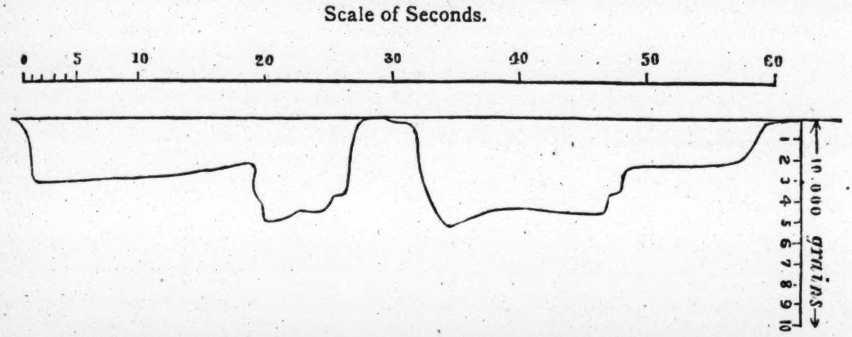
Experiment I.—The apparatus having been properly adjusted before Mr. Home entered the room, he was brought in, and asked to place his fingers in the water in the copper vessel, N. He stood up and dipped the tips of the fingers of his right hand in the water, his other hand and his feet being held. When he said he felt a power, force, or influence, proceeding from his hand, I set the clock going, and almost immediately the end B of the board was seen to descend slowly and remain down for about 10 seconds; it then descended a little further, and afterwards rose to its normal height. It then descended again, rose suddenly, gradually sunk for 17 seconds, and finally rose to its normal height, where it remained till the experiment was concluded. The lowest point marked on the glass was equivalent to a direct pull of about 5000 grains. The accompanying figure (5) is a copy of the curve traced on the glass.

---

* In my first experiments with this apparatus, referred to in Professor Stokes's letter and my answer (page 479), the glass vessel was not quite over the fulcrum, but was nearer B.

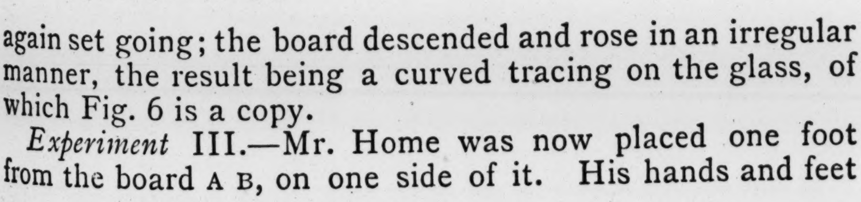
Experiment II.—Contact through water having proved to be as effectual as actual mechanical contact, I wished to see if the power or force could affect the weight, either through other portions of the apparatus or through the air. The glass vessel and iron stand, &c., were therefore removed, as an unnecessary complication, and Mr. Home's hands were placed on the stand of the apparatus at P (Fig. 2). A gentleman present put his hand on Mr. Home's hands, and his foot on both Mr. Home's feet, and I also watched him closely all the time. At the proper moment the clock was again set going; the board descended and rose in an irregular manner, the result being a curved tracing on the glass, of which Fig. 6 is a copy.

FIG. 5.



The horizontal scale of seconds shows the time occupied in the movements, the experiment lasting one minute. The vertical scale shows the tension in grains exerted on the balance at any moment.

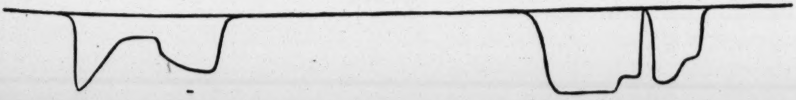
FIG. 6.



In this and the two following figures the scales, both vertical and horizontal, are the same as in Fig. 5.

Experiment III.—Mr. Home was now placed one foot from the board A B, on one side of it. His hands and feet were firmly grasped by a bystander, and another tracing, of which Fig. 7 is a copy, was taken on the moving glass plate.

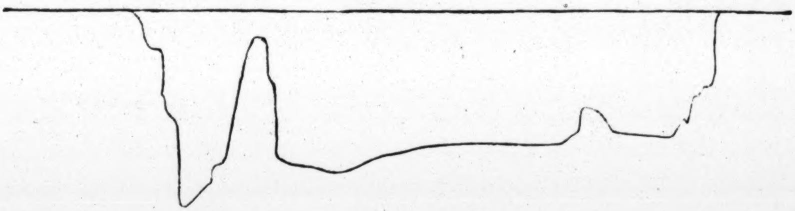
FIG. 7.



Experiment IV.—(Tried on an occasion when the power was stronger than on the previous occasions). Mr. Home was now placed 3 feet from the apparatus, his hands and feet being tightly held. The clock was set going when he gave the word, and the end B of the board soon descended, and again rose in an irregular manner, as shown in Fig. 8.

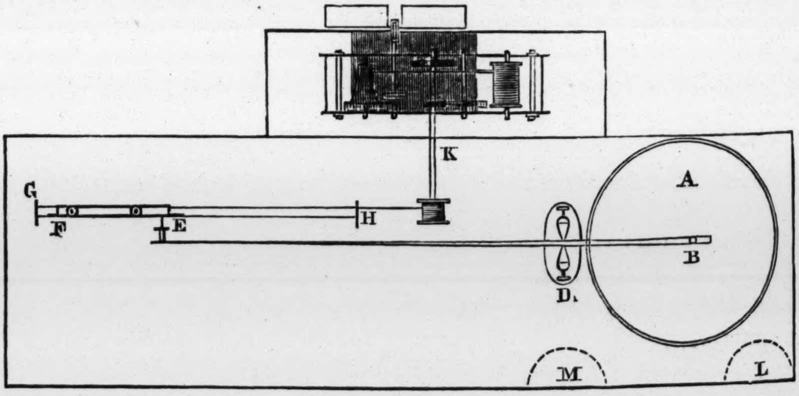
The following series of experiments were tried with more delicate apparatus, and with another person, a lady, Mr. Home being absent. As the lady is non-professional, I do not mention her name. She has, however, consented to meet any scientific men whom I may introduce for purposes of investigation.

FIG. 8.



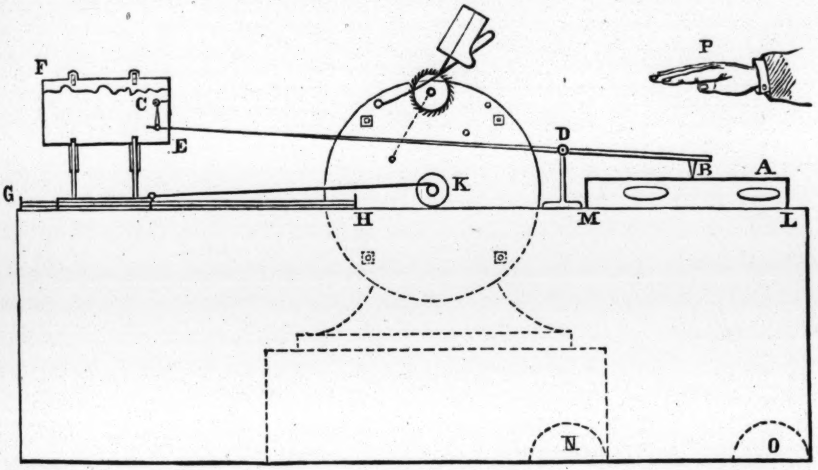
A piece of thin parchment, A, Figs. 9 and 10, is stretched tightly across a circular hoop of wood. B C is a light lever turning on D. At the end B is a vertical needle point touching the membrane A, and at C is another needle point, projecting horizontally and touching a smoked glass plate, E F. This glass plate is drawn along in the direction H G by clockwork, K. The end B of the lever is weighted so that it shall quickly follow the movements of the centre of the disc, A. These movements are transmitted and recorded on the glass plate E F, by means of the lever and needle

FIG. 9. (Plan.)



point C. Holes are cut in the side of the hoop to allow a free passage of air to the under side of the membrane. The apparatus was well tested beforehand by myself and others, to see that no shaking or jar on the table or support would interfere with the results: the line traced by the point C on the smoked glass was perfectly straight in spite of all our attempts to influence the lever by shaking the stand or stamping on the floor.

FIG. 10. (Section.)



Experiment V.—Without having the object of the instrument explained to her, the lady was brought into the room and asked to place her fingers on the wooden stand at the points L M, Fig. 9. I then placed my hands over hers to enable me to detect any conscious or unconscious movement on her part. Presently percussive noises were heard on the parchment resembling the dropping of grains of sand on its surface. At each percussion a fragment of graphite which I had placed on the membrane was seen to be projected upwards about 1-50th of an inch, and the end C of the lever moved slightly up and down. Sometimes the sounds were as rapid as those from an induction-coil, whilst at others they were more than a second apart. Five or six tracings were taken, and in all cases a movement of the end C of the lever was seen to have occurred with each vibration of the membrane.

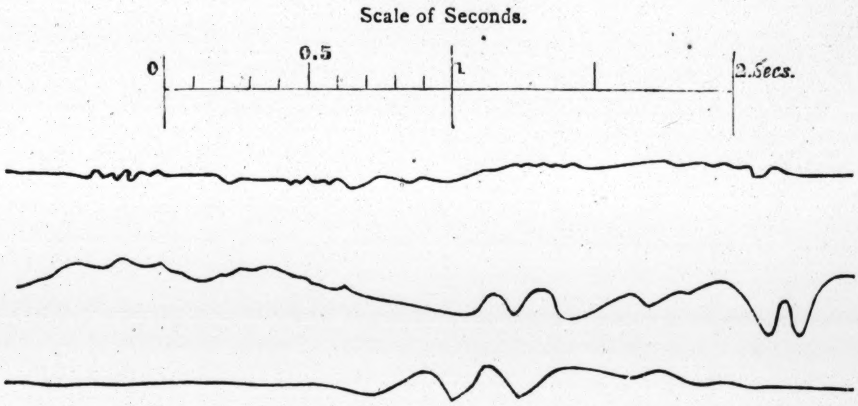
In some cases the lady's hands were not so near the membrane as L M, but were at N O, Fig. 10.

The accompanying Fig. 11 gives tracings taken from the plates used on these occasions.

Experiment VI.—Having met with these results in Mr. Home's absence, I was anxious to see what action would be produced on the instrument in his presence.

Accordingly I asked him to try, but without explaining the instrument to him.

FIG. 11.



I grasped Mr. Home's right arm above the wrist and held his hand over the membrane, about 10 inches from its surface, in the position shown at P, Fig. 10. His other hand was held by a friend. After remaining in this position for about half a minute, Mr. Home said he felt some influence passing. I then set the clock going, and we all saw the index, C, moving up and down. The movements were much slower than in the former case, and were almost entirely unaccompanied by the percussive vibrations then noticed.

Figs. 12 and 13 show the curves produced on the glass on two of these occasions.

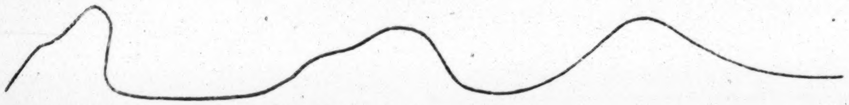
Figs. 11, 12, 13 are magnified.

These experiments *confirm beyond doubt* the conclusions at which I arrived in my former paper, namely, the existence of a force associated, in some manner not yet explained, with the human organisation, by which force, increased weight is capable of being imparted to solid bodies without physical contact. In the case of Mr. Home, the development of this force varies enormously, not only from week to week, but from hour to hour; on some occasions the force is inappreciable by my tests for an hour or more, and then suddenly reappears in great strength. It is capable of acting at a distance from Mr. Home (not unfrequently as far as two or three feet), but is always strongest close to him.

Being firmly convinced that there could be no manifestation of one form of force without the corresponding expenditure of some other form of force, I for a long time searched in vain for evidence of any force or power being used up in the production of these results.

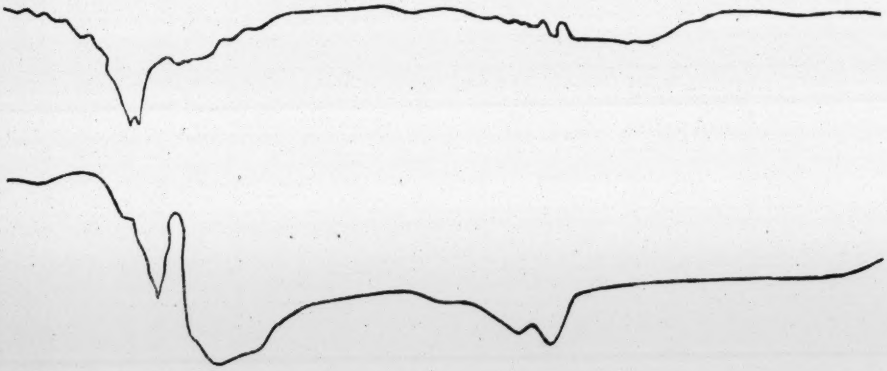
Now, however, having seen more of Mr. Home, I think I perceive what it is that this psychic force uses up for its development. In employing the terms *vital force*, or *nervous energy*, I am aware that I am employing words which convey very different significations to many investigators; but after witnessing the painful state of nervous and bodily prostration in which some of these experiments have left Mr. Home—after seeing him lying in an almost fainting condition on the floor, pale and speechless—I could scarcely doubt that the evolution of psychic force is accompanied by a corresponding drain on vital force.

FIG. 12.



I have ventured to give this new force the name of *Psychic Force*, because of its manifest relationship to certain psychological conditions, and because I was most desirous to avoid the foregone conclusions implied in the title under which it has hitherto been claimed as belonging to a province beyond the range of experiment and argument. But having found that it is within the province of purely scientific research, it is entitled to be known by a scientific name, and I do not think a more appropriate one could have been selected.

FIG. 13.



To witness exhibitions of this force it is not necessary to have access to known psychics. The force itself is probably possessed by all human beings, although the individuals endowed with an extraordinary amount of it are doubtless few. Within the last twelve months I have met in private families five or six persons possessing a sufficiently vigorous development to make me feel confident that similar results might be produced through their means to those here recorded, provided the experimentalist worked with more delicate apparatus, capable of indicating a fraction of a grain instead of recording pounds and ounces only.

As far as my other occupations will permit, I purpose to continue the experiments in various forms, and I will report from time to time their results. In the meanwhile I trust that others will be induced to pursue the investigation in its scientific form. It should, however, be understood that, equally with all other scientific experiments, these researches must be conducted in strict compliance with the conditions under which the force is developed. As it is an indispensable condition of experiments with frictional electricity that the atmosphere should be free from excess of moisture, and that no conducting medium should touch the instrument while the force is being generated, so certain conditions are found to be essential to the production and operation of the Psychic force, and unless these precautions are observed the experiments will fail. I am emphatic on this point, because unreasonable objections have sometimes been made to the Psychic Force that it is not developed under adverse conditions dictated by the experimentalist, who, nevertheless, would object to conditions being imposed upon himself in the exhibition of any of his own scientific results. But I may add, that the conditions required are very few, very reasonable, and in no way obstruct the most perfect observation and the application of the most rigid and accurate tests.

Just before going to press I have received from my friend Professor Morton an advance sheet of the "Journal of the Franklin Institute," containing some remarks on my last paper by Mr. Coleman Sellers, a leading scientific engineer of the United States. The essence of his cricitism is contained in the following quotation:—

> "On page 341" (of the Quarterly Journal of Science) "we have given a mahogany board '36 inches long by 9½ inches wide, and 1 inch thick,' with 'at each end a strip of mahogany 1½ inches wide screwed on, forming feet.' This board was so placed as to rest with one end on the table, the other suspended by a spring balance, and, so suspended, it recorded a weight of 3 pounds; *i.e.*, a *mahogany* board of the above dimensions is shown to weigh 6 pounds—3 pounds on the balance and 3 pounds on the table. A mechanic used to handling wood wonders how this may be. He looks through his

limited library and finds that scientific men tell him that such a board should weigh about 13½ pounds. Did Mr. Crookes make this board himself? or did Mr. Home furnish it as one of his pieces of apparatus? . . . . It would have been more satisfactory if Mr. Crookes had stated, in regard to this board, who made it. . . . Let it be discovered that the 6 pound mahogany board was furnished by Mr. Home and the experiments will not be so convincing."

My experiments must indeed be convincing if so accomplished a mechanician as Mr. Coleman Sellers can find no worse fault with them than is expressed in the comments I have quoted. He writes in so matter-of-fact a manner, and deals so plausibly with dimensions and weights, that most persons would take it for granted that I really *had* committed the egregious blunder he points out.

Will it be believed, therefore, that my mahogany board does *weigh only* 6 *pounds?* Four separate balances in my own house tell me so, and my greengrocer confirms the fact.

It is easy to perceive into what errors a "mechanic" may fall when he relies for practical knowledge on his "limited library" instead of appealing to actual experiment.

I am sorry I cannot inform Mr. Sellers who made my mahogany board. It has been in my possession about sixteen years; it was originally cut off a length in a wood-yard; it became the stand of a spectrum camera, and as such is described with a cut in the "Journal of the Photographic Society" for January 21, 1856 (vol. ii., p. 293). It has since done temporary duty in the arrangement of various pieces of apparatus in my physical laboratory, and was selected for these particular experiments owing to its shape being more convenient than that of other available pieces of wood.

But is it seriously expected that I should answer such a question as "Did Mr. Home furnish the board?" Will not my critics give me credit for the possession of some amount of common sense? And can they not imagine that obvious precautions, which occur to them as soon as they sit down to pick holes in my experiments, are not unlikely to have also occurred to me in the course of prolonged and patient investigation?

The answer to this as to all other like objections is, Prove it to be an error by showing where the error lies, or, if a trick, by showing *how* the trick is performed. Try the experiment fully and fairly. If then fraud be found, expose it; if it be a truth, proclaim it. This is the only scientific procedure, and this it is that I purpose steadily to pursue.

IV. THE RECENT GUN-COTTON EXPLOSION.

It is now some eight years ago that the Gun-cotton Factory at Stowmarket was first established. The great improvements which had just then been completed in Austria in the way of manufacturing the explosive seemed to promise a hopeful future, and inspired chemists and manufacturers with fresh confidence in its qualities and utility. General von Lenk's modifications in the preparation of pyroxylin had been so far successful as to furnish a material at once serviceable and under control, and if the Austrian gun-cotton was not perhaps all that a military or industrial explosive should be, still its complete and perfect elaboration appeared imminent. Indeed, until Lenk took the matter in hand, the employment of pyroxylin as an explosive agent had never been attended with even the remotest success, and our readers no doubt well remember the terrible and frequent accidents that characterised the first attempts to deal with the material in large quantities. Explosions in England, France, and Germany, marked the era of its first short and disastrous reign, and unanimously almost, both abroad and at home, it was resolved to abandon a compound so dangerous and little understood as Schönbein's newly discovered nitro-cellulose proved to be.

Von Lenk paused for a time before bringing the subject again forward, so as to allow prejudice to subside as much as possible, and then with matured plans and fresh experiences sought to obtain further consideration for his *protégé*. Great credit is in truth due to the Austrian Engineer Officer in prosecuting so earnestly his researches with an agent which had excited such universal suspicion and distrust; but fortunately the improvements effected by him were so simple and obvious, that his Government deigned once more to make a trial of the invention. And here, at the risk of being tiresome, it behoves us just briefly to recapitulate in what manner General von Lenk's system of manufacture differed from the ordinary method of procedure. The first form of gun-cotton was, as we know very well, that of cotton-wool, the cellulose being immersed into a mixture of strong sulphuric and nitric acids for a certain period, to effect its conversion into a compound highly charged with nitrogen. Little care was taken to secure a material of the highest explosive force, nor was the elimination from the finished gun-cotton of free acid or organic impurities very strictly studied, so that the result was frequently a product of an unequal and unstable character. The violence with which this material exploded was quite ungovernable, and its lack of uniformity formed an element of risk and danger not easily exaggerated. Lenk's cotton, on the other hand, was a far less treacherous and dangerous article to deal with; the cotton was converted into pyroxylin in the form of skeins or woven twist, and these were treated with strong acids for so long a period, that a more highly explosive, and at the same time more stable and perfectly converted, gun-cotton was the result. Elimination of the acids was secured by immersion of the cotton in running water for the space of three or four weeks, and finally a treatment with silicate of soda or potash followed, which although possibly without effect in retarding the combustion of the cotton—the avowed purpose of its introduction—was doubtless of some importance in checking any incipient generation of acid that might be set up. This yarn gun-cotton burnt with much less violence, and came to be employed with some prospect of success in ordnance and small arms, when twisted and woven into cartridges of different forms; but much more experience of the subject was nevertheless required before it could be safely adopted in this way as a substitute for gunpowder. The military authorities in Austria were in the meantime indisposed to carry on a costly and lengthened investigation, such as a thorough study of gun-cotton promised to be; and as it was evident, even in its improved form, that the cotton was unsuitable for general army purposes, an abandonment of the material necessarily followed.

But the progress made was too valuable to be lost sight of. Scientific men in this country were fully alive to the possible value of this improved gun-cotton as a military and industrial agent, and attention was ere long called to the subject at a meeting of the British Association in 1862, and further investigation of the matter definitely decided upon. Seeing that scientific opinion in England was friendly disposed towards the new explosive, the holders of the patents in Austria at once cast about for manufacturers over here willing to undertake the preparation of the Lenk cotton, and thus it came to pass that presently the Gun-cotton Works at Stowmarket were established for the purpose of preparing the material on the Austrian system. M. Révy, who held the English patent at the time, was, we believe, the projector of the works, which were constructed under his immediate direction, and in a short time the manufacture and application of the material was commenced. For several years affairs went on exceedingly prosperously, if we except one fatal accident, and confidence increased so much among the miners and and quarrymen by whom the pyroxylin was employed, that an enlargement of the Works soon became necessary to meet the increasing demands.

The mode of manufacture at Stowmarket underwent a change in 1868. It will be remembered that soon after attention had been called to the subject at the British Association, a committee was appointed by Government to inquire systematically into the subject. General Sir Edward Sabine, K.C.B., the President of the Royal Society, was appointed chairman of this body, which consisted of several members of the British Association, and officers from the scientific corps of the army. The decision of this Committee was decidedly in favour of pursuing an investigation of the properties of gun-cotton, with a view to its probable employment in military and industrial affairs; and another Committee—that on Floating Obstructions—which included among its members officers of high rank and attainments in the Engineers and Royal Navy, not only confirmed this dictum, but before separating in 1868, expressed a decided belief in the importance of pyroxylin as an agent for mining and torpedo purposes. The favourable opinions thus expressed were, it is but fair to state, based upon results obtained with the compressed or pulped form of the material devised by Professor Abel; and it is not to be wondered at, therefore, that after such favourable testimonies from scientific officers representing every branch of the Service, that the Stowmarket Gun-cotton Company should set aside the Austrian mode of manufacture for that of Mr. Abel, which had been so universally commended.

Of the safety of manufacturing this compressed gun-cotton, and of its perfect stability when properly prepared, there can hardly be a doubt; but that our readers may judge for themselves of the manufacture, we will briefly detail the points in which the process differed from the earlier method of Von Lenk. Instead of the costly yarn previously employed, a good quality of cotton-waste was used for conversion into pyroxylin, and this after being steeped for some forty-eight hours in the acids, was well cleansed and subsequently beaten into pulp. In this form it was put through the poaching process, where by means of paddles the disintegrated mass was perpetually kept suspended in water until all trace of acid or other impurity had disappeared, and then a treatment with carbonate of lime followed to impart an alkaline tone to the material. The pulp was afterwards pressed into discs or cakes of any desirable form, and dried upon wire nets at a somewhat high temperature. It is not until gun-cotton of this kind is heated to something above 300° F. that any trace of nitrous fumes are perceptible, and its actual igniting point is seldom below 350°; for, as will readily be understood, the exceedingly perfect washing process to which the cotton is subjected must infallibly remove all trace of free acid.

We are compelled to make these few preliminary remarks before approaching the subject of the recent explosion, in order that our readers may fully understand the actual *modus operandi* adopted at Stowmarket, and at the same time be cognisant of the circumstances that seemed to justify the Gun-cotton Company in undertaking the manufacture of this powerful explosive. The confidence expressed by the many eminent authorities on the subject who composed the committees of which we have made mention, certainly constituted a very substantial guarantee of the comparative safety of the material; and, moreover, the practical experiments made with considerable quantities of the material by the Royal Engineers, and mine and quarry managers, for several years past, confirmed the favourable opinion formed by scientific men.

Under these circumstances, then, it is not surprising that the Stowmarket Company prosecuted the manufacture of gun-cotton, and that, alive to recent improvements, it adopted in later years the simple and ingenious modifications elaborated by Professor Abel, whose persevering and exhaustive investigations are well known to all of us. Year by year the fame of gun-cotton as an invaluable mining agent spread abroad, and as a new and particularly effective method of removing submarine rocks and sunken wrecks, as likewise for employment in torpedoes, it has stood for some time without a rival. It should be borne in mind that a more powerful explosive than gunpowder is in many warlike matters simply indispensable, and military men were indeed delighted to find in gun-cotton an agent which, while free from those risky attributes that characterise most of the violently detonating compounds known to chemists, was at the same time possessed of such extraordinary force. Official committees had lauded its qualities; military men without exception were satisfied with its practical results; for industrial purposes it was extensively employed; the scientific reports were unanimously in its favour; and in face of these circumstances, Government very naturally decided upon its adoption into the service.

In the midst of this comes the appalling explosion at Stowmarket. An establishment covering some three acres of ground, and consisting of factories, outhouses, and magazines, so built that the group of buildings presented the appearance of a little model village, has been literally scattered to the winds by the ignition of some ten tons or more of our new military explosive, and this just as the consummation of its success was at hand. Where the principal magazine once stood, an extensive pond 15 feet in depth is now to be found, located, it would appear, in the middle of a ploughed field, for so complete is the annihilation of the building that scarcely a shattered fragment remains to mark its previous whereabouts. The workshops and outhouses around present one mass of ruin and desolation, and the strong iron shafting and metal piping have been dashed into fragments as numerous and small as those of the most fragile portions of the structure. Some of the roofs and walls are literally shivered into atoms, and the only whole pile remaining is singularly enough a tall brick chimney. When we state that out of 130 men, women, and children that were employed upon the premises as many actually as 100 were killed or wounded, the destructive nature of the catastrophe will indeed be fully understood.

To what cause is the explosion to be ascribed? Shall we straightway determine once for all to give up gun-cotton, which as long as it has been known has always borne the character of a faithless and unreliable servant, and thus have done with it for ever? Shall we discard it as a material whose nature is too little understood, and whose behaviour we find it impossible to control? But if we decide on such a course, then to what substitute shall we turn our attention? —to nitroglycerine, dynamite, picric powder, or some of the fulminates,—for compounds of this sort we certainly must have for warlike purposes. Truly, we cannot but hesitate before adopting any of these latter explosives, for in many cases their stability is, we know, more than doubtful, and thus we shall do wisely to make at any rate some inquiry into the cause of the terrible event, and not to throw away ruthlessly all the information and experience that has been acquired during the past fifteen years.

In the first place, however, irrespective of the actual cause of explosion, we cannot admit for one moment that there was any reason on earth why its effects should have been so disastrous. We have here a store of a dozen tons of dry and highly explosive gun-cotton within a few yards of a populous factory, where, indeed, not one ounce of the material need have been stored. Nothing but wet, and therefore uninflammable, cotton should have been in the factory, for the drying, as also the storing, might have been carried on just as well many miles away from the buildings. It is nothing but this silly overweening confidence in the safety of gun-cotton that has caused such widespread destruction; for when it is remembered that the simple infraction of a rule by a careless workman would have been sufficient at any time to have caused ignition of the mass, the callousness of the authorities at Stowmarket is simply astounding.

The accident, has, however, happened, and it remains but to find the cause. In this task we have fortunately some slight clue, and one which we doubt not will afford a satisfactory explanation of the whole affair. The Gun-cotton Company had recently entered into a contract to supply Government with the material to the extent of 200 tons. Of this quantity two deliveries, each consisting of 10 tons had been made, the last supply singularly enough having been delivered to the Government authorities on the very day of the explosion at Stowmarket. The cotton had, in fact, only quitted the Company's magazines but a short time previously, and consisted of identically the same description of material that exploded with such disastrous results. Under these circumstances, supposing the cause of explosion to have been due to the gun-cotton itself, it follows that an examination of the store now lying on Government premises at Upnor Castle would furnish important evidence in the matter.

By order of the Secretary of State for War an inspection was accordingly instituted at Upnor, and to prevent in the meantime any repetition of the disaster, the disks or cakes were at once saturated with water. The boxes were opened one by one, and the contents carefully examined: for some time nothing of a suspicious nature was to be observed, but presently a few of the packages were found (containing material only just received from Stowmarket) which enclosed disks here and there very strongly tainted with acid. Not only did litmus paper readily proclaim an acid reaction, but both eyes and nose at once bore evidence to the fact, so marked was the presence of the free acid.

Here, then, was no doubt the true solution of the difficulty, for it is well known that impure and acidified gun-cotton packed in close localities and heated continuously for some time is prone to rapid decomposition, followed after a time by spontaneous ignition. A small quantity of bad cotton would not only decompose of itself, but likewise contaminate and perhaps set fire to good pyroxylin in its immediate neighbourhood, so that even minute charges of the inferior material are necessarily very dangerous in a confined magazine. The evidence we possess of the state of the magazines, roofed in with slate, and exposed for days to the full glare of an August sun, seems quite sufficient to us to account for the decomposition of any impure gun-cotton, and of this there was evidently a goodly portion at Stowmarket, seeing that the Government supply was admitted to be of a picked and specially good nature.

The manager of the works, in reference to the disaster, speaks, it is true, of two separate events, the first of which he specifies as an explosion, and the second as a detonation of fearful force. Another witness testifies to seeing a flare-up previously to the report, and both these statements are doubtless quite correct. It may have been that a detonation of some portion of the cotton followed the first milder form of explosion, and wrought a great part of the damage; but in any case the primary phenomenon was the spontaneous ignition of a large mass of the cotton, and we may confine ourselves, therefore, to seeking the reason of this first catastrophe.

It is, we believe, conceded by most of the witnesses, both scientific and unscientific, that the cause of the calamity was certainly due to the presence of free acid in the compressed cotton, which should instead have presented an alkaline, or at any rate a neutral, reaction. How did this acid, then, come into the cotton? The explanation received by the jury, and which can scarcely be deemed satisfactory even by its advocates, is that the acid was wilfully and deliberately put into the cotton by some disaffected person, although, as the coroner could not help admitting, there was singularly enough no motive or suspicion pointing to such an infamous act. To us, we must say, there seems as much direct evidence to warrant this belief as there was to support the hypotheses that the explosion resulted from the falling of a meteoric stone, or from the act of a foreign emissary, both of which plausible solutions have been suggested. True, a certain amount of sulphuric acid was found in portions of the bad gun-cotton, which could not of course have existed therein if the material had been but tolerably washed in the first instance; but it is surely somewhat rash on this frail circumstantial evidence alone to deduce proof of foul play. If the catastrophe can be explained in any other way, why is it at all necessary to bring a charge of so grave a nature even against "persons unknown;" and that other explanations are possible, and probable, any one who is cognisant with the manufacture of gun-cotton must admit.

All agree, we repeat, in saying that there was free acid at any rate in some samples of the cotton, and, moreover, we have it on record as a positive fact that gun-cotton in a state of decomposition will explode spontaneously when exposed for some time to a high summer temperature, a circumstance which has been scientifically demonstrated beyond cavil. Can anything, then, be more conclusive? We think not; and now there remains to be explained the difficult matter—how the acid got into the cotton and thus set up incipient decomposition in the first instance.

Putting malice altogether on one side, the presence of acid in the cotton may be due to two causes; either the acid was never perfectly removed in the poaching process, or in drying the discs became overheated and nitrous acid liberated in this manner. Although we do not by any means incline favourably to the latter explanation, still the account given of the manner in which the cotton was dried shows that the process was carried out in a very loose and careless manner, and that instances were not rare of the ignition of the cotton on the frame by overheating. From the drying-house the gun-cotton was at once sent into the magazines without further testing—a grave oversight we cannot but think—the material being subjected to examination only when in the wet stage of manufacture.

It was during the actual manufacture of the material, the evidence seems more conclusively to point, that the mischief originated. The testimony of the workpeople proves beyond doubt that the manipulations were of late conducted in an unusual hasty manner, and that vigilant care and attention, so essential in a manufacture of this kind, was by no means exercised. Instead of dipping only 75 lbs. of cotton per day, which would seem for some time past to have been the regulation quantity, double that amount was expected to be treated by the labourers in the dipping-house, and as a matter of consequence acceleration in the conversion process signified also the shortening of the washing operation. If properly conducted, it is difficult to see how any trace of acid could possibly remain in the cotton, for the precautions provided for its elimination were as perfect as could well be devised. After centrifugating to remove the greater portion of the acid, the dipped cotton was placed for some time under a formidable cataract, which literally beat out the acid from the fibres; breaking up and tearing into pulp in a plentiful supply of water followed, and in this finely divided state it was churned or poached in another bath for many hours or even days. During this process also a small quantity of chalk and lime-water was added to impart an alkaline rather than an acid reaction, and it was at this stage of the manufacture that samples were always taken for testing. Indeed, until the chemist reported the batch to be free from all trace of acid,—for it appears acid was often found in the poachers,—the poaching was constantly proceeded with, and not till it had passed the various tests satisfactorily was the pulp pressed into shapes and dried.

The increase of work at Stowmarket was due to the execution of the large Government contract to which we have alluded, and it is to be feared that press of business created a lack of vigilance upon the part of the foremen and others. We have it in evidence that on one occasion a batch of very inferior cotton was in some way or another actually finished and dried, and although the circumstance created at the time much concern among the managers, still no satisfactory solution of the affair could be suggested. The cotton was supposed to have passed rigidly through all the operations, and stored in the ordinary manner, and yet on subsequent examination it was found to be fully impregnated with nitrous fumes and in a condition similar in all probability to that presented by a few of the discs delivered at Upnor. Inasmuch as the chemist whose duty it was to examine the samples protested that his tests had always been carefully and critically applied, we must suppose that either some mistake occurred in sampling or that no specimens of this particular batch had been sent to his laboratory, but that it left the factory without being properly washed and poached. What happened once may happen of course again, and in the hurry and bustle that unfortunately but too evidently existed, there is no material doubt in our mind but that an impure batch of gun-cotton—the charge from one of the poachers probably, weighing about half a ton—was passed into the magazines.

It is much to be regretted that better scientific supervision was not exercised in a matter so purely technical as that of manufacturing gun-cotton. The particularly satisfactory nature of recent experiments carried on at Woolwich in regard to the combustion of gun-cotton under ordinary circumstances seems to have led, if not to a callous, at any rate to a too confiding, belief in the harmless nature of pyroxylin. Such experiments were, of course, not without value, but a store of some 15 tons of the explosive, heated for many days in a tropical sun, was hardly likely, if accidentally ignited, to burn away harmlessly in the same way as half-a-dozen boxes of the material put into a bonfire. An explosive of any kind, especially when stored in large quantities, must always be regarded as a most eminent source of danger, and it is not only foolish and wrong, but positively criminal, under any circumstances to treat it otherwise. During the different stages of manufacture constant attention is indispensable in watching the gradual and important changes that the cotton undergoes, and this attention should be given not only by a skilful chemist, but by one specially learned in this particular branch of chemical knowledge.

In conclusion a few words are perhaps necessary touching the stability of pure gun-cotton, for a sweeping explosion of this kind cannot but shake in some degree the confidence that previously existed in the permanence of the material. The best proof that can be afforded is, we think, the fact that quantities of gun-cotton, not pounds, but hundredweights, have been stored up for the last seven or eight years in ordinary Government magazines exposed to all weathers, both summer and winter, and this material without exception is proved to be as sound and uniform in appearance as on the day of its manufacture. Moreover, gun-cotton from the Stowmarket factory has been stored, transported, and used since the years 1863 and 1864 both by Government and private individuals in very considerable quantities, and, so far as we are aware, no authenticated accident from spontaneous decomposition and ignition has arisen. If we add to these practical results the experiences of those chemists who for the last ten years have made pyroxylin their especial study, it must be acknowledged that there is as yet no real evidence to contradict our present views, and we shall deeply deplore the circumstance if the panic now existing has the effect of hastily throwing on one side the valuable information which has only been acquired after so many years of laborious investigation.

V. THOUGHTS SUGGESTED BY PATENT RIGHTS.

THE Royal Commission on Patents still continues and must keep many persons brooding over the subject. We cannot entirely forget it. It reminds us, as indeed every question in social life now does, that new foundations are sought for our institutions, and every new part, if only a chamber or pinnacle, must stand independent of the old, lest it should be disgraced by a too close connection with an antique model. Patents confer rights defined by words and not by natural borders of mountain or shore. Their extent cannot be measured by chains, and we can only see it by understanding clearly the spirit of the men who made the laws and of the country which upholds them. But in these days we ask, what right has any man to exclusive privileges? We certainly will not discuss the rights of man. It is our belief that nature has not yet defined them to us in their fulness, and we wait for the revelation. We know, too, that man has not been able to define them to his own satisfaction, except when self has been too much the master. But we certainly think every man wrong who imagines that these rights ought to be the same in every state of society, and who denies that laws must change their form according to our progress. It would be sad, however, if this change extended to the very spirit of the laws themselves, and if the principles of justice were to alter in the very minds of men. It would be still harder, however, if men were to imagine that perfect justice was consistent with perfect truth or with the nature of man. All great forces are tempered by other great forces, or the universe would be really monotonous. A certain hard reasoning in our times seeks for pure sharp justice everywhere; and mercy, which never will cease to protest with perhaps equal power, is called by some narrow-minded men sentimental.

The utilitarian looks only to that work which produces money or goods, and forgets that man's life may be better destroyed than deprived of that which may be called sentiment. We cannot call the feeling of a desire to be useful to society mere sentiment, but we may call it sentiment of a dignified kind. When a man labouring for society is neglected and despised, his disappointment is not mere sentiment, but it is a sentiment arising in the noblest minds which history has seen. To a great extent our happiness arises from our sentiments even in the lower ranks of civilisation; but in the higher ranks, where the external is cared for, the whole burden of man's life must lean on his sentiments only. We know that some men will give us various meanings of the word sentiment, but taking the meaning so abundantly given to it, we are justified in the assertion.

It is not the belief of all persons, but we think most will agree, that man is entitled to push forward his rights into the external domain of nature to the utmost of his power: when nature objects he is easily repulsed.

We know nothing to limit him but his capacity. Regarding his rights over other men, we should probably have said the same thing had we lived in the time when to conquer nations and to keep slaves was to be thought great; but we now learn from history that the rights of a man are limited by the rights of his fellow men, whether it be in a hand-to-hand struggle, or in the region of thought and feeling only. After leaping over several syllogisms we may from these data decide that patents, like every other desire of man, must be bounded by the desires of other men, and that justice in granting them must be tempered with mercy for the weakness of the individual struggling for self against a nation. To save time let our readers connect for themselves more fully these fragments of thought.

There are men who prefer the purely utilitarian view combined with the national, calculating the results in wealth obtained for the nation as a whole. They look at the statistics of trade and the revenue derived from income tax, and draw conclusions which seem hard to contradict. This view overrules at present, but we have surely learnt that great wealth in a nation may co-exist with great misery, and great appearance of power with great weakness. The true prosperity of a nation must consist in the progress and happiness of the individuals. It is very difficult for us of late to avoid centralisation. We think over all the country on the same subject every day; and when great undertakings are demanded we rush to co-operate, and the work becomes to a certain extent national, however nominally private. By this means we have been able to do great works, and the railway and steamboat systems are wonderful examples, whilst co-operative societies are forming to protect the individual in the smaller details of life. We must rejoice to see the work done. So thoroughly do we seem satisfied with it, that we seek to train the minds of men in the same wholesale way;

and we have a central University of London seeking as much as possible to make all men of the same standard, expecting thereby to carry out the utilitarian national method and obtain the most out of every man.

We object to all the modes of treating men if carried out singly. We object to justice dealt out with mathematical accuracy—for in that case who would stand. We object to an intrusive mercy which would weaken the minds of men, and which elicits that abundant false sentiment too often used as an argument against sentiment the most ennobling. We object to measure the happiness of nations by their wealth and by their power; and we object to any mode which turns many individual minds in one direction unless for some special purpose, instead of leaving them to develope naturally; we are not creators, and do not know what is best for the whole race as well as each individual.

Every one must see the wonderful advantage gained by the union of many individuals for one purpose. We admire it even when it is connected with a debased machinery which makes railways for the purpose of benefitting a few individuals, because nature is higher than they, and the community gains in the end. All men have agreed that Æsop's bundle of sticks teaches a lesson not to be gainsayed. But there are many of us who see in great combinations too much one-sidedness of thought, and prefer to see combination confined to such departments as cannot be managed by a few.

Applying these ideas to the Patent question, it would appear as if there existed an opportunity of encouraging many centres of thought and action, and of counteracting the too great unity of thought and feeling, introducing itself by our latest forms of teaching and examining,—a system apparently borrowed from France, where it has given an unmanageable amount of activity to the capital, and done much to annihilate the independence of the provinces and of individuals.

It is our duty to encourage individuals in the course of thought which is peculiar to them if it offers any advantage to mankind. We are in danger of directing the national mind. Direction is good for the young to a certain extent, for a nation it is dangerous; if it is against the natural impulses it causes curious twists and misshapen growths. Who knows except by looking at the past the probable course of humanity? And who can draw the line of the past straight so as to know its direction? And who knows whether nature intends that the path shall be straight, or whether the simile is wrong, and we are in the trunk of some tree of civilisation or in one of its small branches? There are men and societies who seek to direct the line of invention, and offer prizes; and within certain limits the system answers, but it is for details. The want of great results from this plan is marvellous. We may well say the wind bloweth where it listeth: how little can we even control our own minds. The result is not exactly that which we wish when we begin a study, it is sometimes far beneath our hopes and sometimes far above them. A poet expressed thus the influence of the unknown upon him, "Perhaps it may turn out a song, perhaps turn out a sermon." And we may join to this words from a very different source, "So it is with every one who is born of the spirit." If the glow of genius be in him—if he be beyond the range of thought given him at examinations—he will go fearlessly where his sails carry him, and he will discover new lands; but if we follow him and direct him he may lose the way—the inner guidance under which he is may be lost, as a tune may be in a crowd of discordant sounds, and the real distinctive part of the individual may never be recovered.

It seems better that in such a case a man should have his own way, and a patent frequently suits well, allowing him time to work and to keep himself by his work, when he has a useful thought promising good to some extent at once, and encouraging him to produce more. There are men who imagine that honour is sufficient, and that an equal number of inventions would be made if no patents were allowed. They make a confusion of thought. A man gives out a scientific discovery for the honour, but so far from a man taking a patent for the honour, most persons are ashamed of them and take them simply because of the money, suffering rather a certain amount of scientific obloquy. If the patents were not granted these inventions would not be made. However, this is not a matter of reasoning. We who have lived long among scientific men and also inventors in the arts more or less scientific know it to be true. Numerous inventions have ceased to be carried to completion because of the uncertainty of patents, and because of the frequent robberies committed by the numerous highwaymen who especially prey on such wealth. We must not forget the stimulus of honour, it would be better to see it more closely allied to patents; but the material weal is so dear to man, that we err still more by forgetting it. Let us forget it in the case of the scientific man himself who works apparently for pure honour. Still how many even of these would work if no universities gave them a living? We must not look too near; the number of men that work at science and make no living by it is not great. Even literary men whose work is pure thought, do they not make their thought a business? And are not the poets of the highest class ready also to receive their reward from every new poem and new edition, adding to their fields and flocks and herds? Have the materialists and utilitarians suddenly found that there is a class of men called inventors to whom money is nothing,—a class that will be satisfied with honour, and so be made to work for the benefit of the nation at a very cheap rate. It is the old principle, "So long as the poor slaves do our work for their living only, let them live like the lower animals. We shall take their work until their friends raise up a great emancipation-explosion. If they must be free we shall try what can be done with coolies. Or if we must give them land they shall be 'bound to the soil,' and work for us free a part of the year." But freedom finds them out. The masters, however, are on the alert. "Let us give them wages enough to keep them well." This is done, and the days of good wages arise because good wages bring good workmen in some places. "But let us keep them to their own work; let us give them no education lest they rebel; let us not teach them the trade lest they be independent; let us teach one to tie a thread and one how to wind a reel; but let us not teach any one all the art." And so these uneducated fragments of machinery are tossed about our great towns, until they are caught in the central whirlpool, diminished in body and stunted in mind, sickly, dirty, and short lived; they keep whirling nearer and nearer to the great gulf, until the outside world raises up friends and demands universal education.

We scarcely expected that the spirit of the task-master would find a resurrection in a new and parliamentary form, and seek to make slaves of the inventive genius of the country, saying, "Work for me and find your own living." This is the lowest stage of slavery—it is done by the despot who has conquered: he refuses even bread. He counts the cost thus, "What shall I gain if I do not feed these men, and how much shall I gain if I feed them?" This is the worst utilitarian-national view. This is what some people call taking a wide view of a subject. It is known to be a narrow view; it is known that good wages and good work are most advantageous, and necessarily must come together in time. Yet these men who are studying politics see only the narrowest application of the principle. The age is famous for an immense number of very long, narrow views, and individuals imagine their own to be wide because of this. The age may be, but all views from one point only are narrow; a man may even study astronomy all his life, and be as narrow-minded as he who studies only the making of telescope-tubes.

We have tried very hard to understand why a man who cultivates mechanics for years, and learns how to apply its powers in a new way, should not have the value of his labours preserved from robbery as well as he who has cultivated an acre of ground in an old-fashioned and imperfect way and sown it with potatoes. But one of the great arguments used by the opponents of patents is still more wonderful. It is believed that the course of science and discovery is so straight that we have only to move on and we shall come to everything. If one man does not gain the new idea another will. If this is true of anything it is true of land. Somebody was sure to find out the unknown acre soon, and now we reward him by the firmest of all possessions. Now we are sure of the acre being discovered, but not sure of the mechanical invention. The number of actual inventions is small. We have seen for a life-time the discomfort of men at coach-offices and railway-stations who are obliged to use many small tickets, and to tear them from a printed sheet containing large numbers; they generally took an opportunity at a moment of leisure to cut a dozen or two nearly off, leaving only a corner attached. It was long before the idea of having them nearly cut by puncturing came into any one's mind. And yet the opportunity was before the eyes of millions of nineteenth century men. Some other man than the actual patentee would have found it unquestionably, we believe, but it stood undiscovered for many periods of fourteen years. Is it a great reward to give one the benefit of it to himself for one such period? The case is quite obvious, and we might rather say why should it be given for one such period only. We may ask in connection with it, for how long shall a man be allowed to cultivate his field? For how long shall a landlord refuse to improve his land, diminishing the food of the country? We should allow him to continue till public opinion acts on him or his successors, but we should do it consistently, and not as the opponents of patents.

It has been said that if a literary man gives an idea in an essay or a poem every one may use it, and the patent right (or copyright) extends only to the form. We do not call this an argument at all. A literary work frequently has its chief, sometimes its only beauty, in the form. What is a poem or a drama without the artistic form and form of expression, or what is any work of fiction or historical work apart from it? If a writer uses expressions from Dickens or from Carlyle—both men whose style is peculiar—it does not diminish the value of their works; on the contrary, it excites the desire in many to see them, and renders the copyright more valuable. Every author delights in being copied and imitated, and if it should be done secretly others have a delight in showing it clearly. In the copyright law we have given literary men a right as definable as the right of land; are we to refuse a similar right to those who work in the applications of science?

To our mind the only defensible objection to patents would lie in the impossibility of defining them if this existed. We throw aside as pure rubbish the objections made by manufacturers in the necessity of paying for patent rights, as we throw aside the arguments of the Americans, who object to pay for our copyrights, and so pirate our books. It may be difficult to define, but law itself is a difficulty daily increasing, and the rights of workmen of every class, even the rights of the House of Lords itself, are continually under discussion. Yet the work of law must be done, unless we fail in our interests and make outlaw classes from sheer inability to tell how to protect them. To examine this supposed difficulty connected with patents, we must recur to the supposition that there are many persons at one moment rushing on an invention, and that by the laws of progress it comes without special merit of one individual. We can easily imagine this to be the opinion of men not thoroughly conversant with the whole subject. There are a class of secondary inventions of which this is strictly true. Let us take for example a very remarkable discovery, marking distinctly an era not long past—that of the first aniline colour by Perkin. If he did not deserve to have that preserved to himself, it is hard to see how any man ought ever to have any possession whatever. Let us imagine for a moment that part of the work was accidental, that does not matter; it is such an accident as does not happen to fools. Besides, if it were wholly accidental, we may say it is perhaps by an accident that a man becomes a king, or a wise man, or an idiot; but he is no less such, and we leave such in pretty firm possession of their characteristics, although their rights are variously disputed. This first discovery by Perkin may be called a primary one; it begins a new class of bodies. After its mode of operation was pretty well known, it was not very difficult to obtain a large number of other bodies more or less allied. These are evidently secondary discoveries, and in them the chief difficulty of the law rests. As dependent processes increase around a primary they become easy; they may come into the hands of men who have little science, and the latest may be the best in a mercantile point of view. What is to be done with this last; is it to be ignored? Here comes in with great propriety the utilitarian national view. These later often come into the hands of wealthy and energetic men, who see that facility and cheapness of manufacture have been attained, and now is the time to launch out money. The people then benefit, and the national advantage of patents is gained to the utmost. Surely it is justice to the inventor that these subsidiary processes shall not overpower his, and it is justice to the nation that the inventor should not have the power of limiting the expansion of his invention. This point was really the greatest difficulty in patent laws, but surely the acumen of legal and scientific men will overcome it, so as to do justice both to the real inventor and the improver of the invention. We are greatly ashamed to see the many little shifts whereby a patent is evaded. Sometimes the real value of the patent is in the result and not in the method. There may be many ways of attaining the same end. We see no reason in such cases for refusing the patent to the result; let the grant be as a reward of ingenuity or wisdom, and not a narrow-minded attempt to obtain the most and to give the least. A scientific judge would probably throw aside most of the attempts at robbery by men who use the law to break it; but we do not see how one who has not studied science can see all the bearings of the subject. The writer of a previous article in this journal was severe both on legal and scientific men; but he ought to have shown more clearly that their faults lie where they interfere with each other, because in the imperfect education of both they are each unintelligible to each other. In a scientific application some might prefer the judge to be a scientific man with a wide education if such can be got in sufficient number; they are very rare indeed who can think otherwise than in their own grooves, and the best would require a legal assessor of course.

It must not be supposed that we neglect the utilitarian national point of view; we object only to its exclusive use. Sometimes it is convenient to look at the sun through a pinhole, and the wisest astronomers look at it through little tubes, or examine its spots projected on disks; narrow views are for certain occasions the best, but the highest glory of the sunlight or of a man is not best seen by dissection. We cannot forget that when a nation is in danger private interests are counted as nothing: so if any man will prove that the labour of individuals in the direction of invention in the arts must be treated on different principles from other labour because of national necessities, we must yield; but in yielding say that the hope of the state must be temporary only if it depends for progress on men who are to be robbed of all their wealth of invention as soon as made, and irritated by their belief in the injustice of their treatment. By some strange delusion some men believe that inventors will make quite as much money without patents as with. According to a quotation in a recent number of this Journal, it would seem as if Count Bismarck believed that by excessive sharpness it might be managed, but the inventor must be sharp indeed if he is sharper than a thief of inventions. If the profits of an invention are divided over many instead of given to one, each must receive only a small share. This is not a matter to reason, it is certain. The inventor cannot by any amount of sharpness keep before all his opponents for fourteen years. They may begin where he leaves off, and without any trouble at all they are up to him. Man may advance in knowledge, but he cannot advance in time, he must live to-day; no amount of sharpness will ever lead him a moment nearer to-morrow than it will his dullest neighbour. And why should a man require to be sharp in order to obtain justice or in order to make a living? It will be a sad world when only such persons can live. Must we give up punishing thieves on the plea that if we had been as sharp as they we should have lost nothing. It is acknowledged that an individual cannot as a rule defend himself in a great community, and therefore we have laws and law officers. These are for the protection of men not sharp enough to protect themseves; the very sharp people would rather be without them.

Let us not forget the assertion under examination, that inventors would make as much without as with patents. As a rule inventors have not money; this might have been used as a powerful argument to show that inventions were made for the sake of the money. It is, in fact, a rare thing for a rich man to enter into the society of inventors; he may continue to invent after he has entered. The idea among them is "Let him take castles who has ne'er a groat."

Now, if all men were honest, patents would not be required, truth would be equal to a patent for the whole life. But if all men were honest and good, what would be the use of policemen or criminal courts, judges or barristers? and how small would the work be for arranging matters in a friendly way in our civil courts? He that has money will carry out the invention to most profit; and we know from daily observation that unless he is obliged to pay the inventor he will not do it. Now there is no use in denying these things, they are well known. If inventors were to be so foolish as to go on inventing, and to be robbed without any hope whatever, they would be much duller than we imagine them to be. We leave with little attention the question whether inventors work for simple honour. The very fact that, whenever an invention, scientific or otherwise, can be used for making money, it is patented, is a sufficient answer. But supposing it to be true that honour only were wanted, it would be still an argument for patents. Men would then receive their proper place as inventors and be registered as such. Although we know that this would not satisfy them, it may be called supplementary reasoning. A good guide to the inventions made would be invaluable, and if it were authoritative still more so. A good beginning is made in the abridgments by the Patent Office, but we are sorry to say it is far from being a record of purely honourable achievements; we should be very glad to see it weeded so that reading might be less wasteful of time.

We may now look at the method by which the patenting stage of an invention is arrived at. Some people, especially such as have read about inventions and discoveries in past times, and who have impressed on their minds stories from their school books about glass being discovered by accident when some people made a fire to cook by on the shores of Syria, and about hens with clay on their feet going over some sugar, have some idea that inventions are generally accidents. If we examine the history of the two inventions alluded to above, that of making glass as it is now required to be made, and that of purifying sugar as it is now purified, we shall find that each subject requires a large portion of a lifetime for full understanding; nay, more, if we look at the history of the men who have made certain of the inventions included in the history, we shall find that they have devoted the greater part of active, intelligent, and valuable lives to one small portion. Such we assure the men who believe in these typical inventions-by-accident, is the truth concerning the progress in the arts. For glass we must have something more than a wooden fire under a sand pot in a desert; we must have the fine adjustment of the silica to the soda and the potash, and the lead or the lime; the fine toning of shade at times by manganese; the colouring by oxides of copper, of zinc, of uranium, and other metals; the Siemens's furnace for the large pots, and the heavy machinery for lifting the melted mass pouring on the thick iron tables, and rolling it; the fine arrangements for annealing at a proper temperature, and the machinery as well as the choice of sand, emery, and oxides of iron for the polishing. Then the blowing, colouring, cutting, moulding, designing. We shall not enumerate all the complicated labours of sugar refiners. The hen and the clay have long been forgotten, and the sugar refiner must use the latest inventions of science, namely, the newest forms of polariscope, and the finest chemical tests to examine the samples in order to see whether the sugar is worth buying and worthy of his labour. No accidents help men as a rule in inventions, except such as happen to all men who are ready to see the phenomena of nature as they occur. There are a few accidental appearances, slight openings into unknown chambers of nature and art, which are shown but for a moment as if some stone had fallen down and the slaves of nature had not had time to cover it; but as a rule inventors dig and fight these slaves on their own ground until by conquest the two become friends. We could tell of many patents that required years of labour before they were fit for the public, and even of some which were the result of the experience of half a life. We could tell also of others taken by two persons in the same month; of these many came to nothing. One, as an instance, was an idea caused by the general current of thought regarding atmospheric railways, and the people preferred to have the patent first lest it should be lost to them, so much do inventors live in terror of pirates. We are led again to this point, Do individuals think, or does the community? We agree with the writer in the January number of this Journal, "It is to the individual that nature and providence give the rich rare gifts that advance humanity. The history of the few has been the history of human progress; and a lost thought may roll for ages through creation without finding a mind to comprehend it or a brain to make it useful to society." Had Horace not lived, who would have given us his charming odes? would they ever have been written by Virgil instead, in order to make up for the want of a Horace? Who would have given us the rich gossips of the east of the Mediterranean if

Herodotus had not done it? No man has a right to say that it ever would have been given; we believe it would have been lost as the history of most other times has been lost to us, leaving only the scanty pickings of the scholar. Had David not written the Psalms the deepest insight given to us of the human heart would have been lost. Providence does not send one man immediately to do the work of another who has been neglected. If the world neglects its benefactors it must suffer, and it does suffer. This is most assuredly true of the great men, the prophets, the sages, the wise rulers, the great poets and historians; their wisdom lies for ages unnoticed. The question arises, does the same argument apply to science and to the arts? It does, although not with equal power. The reason of the inequality lies in the line which science has drawn for enquiry, a good deal of the road is already roughly made a-head of the part where the side-lands are inhabited. But only roughly made, and we frequently err by fancying that it is made at all, and he who goes quite off the common track is often the most successful. Had Faraday not lived we have no reason to believe that our knowledge of magnetism would have been where it is—we are almost certain it would not. Had Joule not been born it is equally certain that our knowledge of heat and force would have been in a very dim state. We see no men that were inclined or able to take his exact place. In the applications of science Davy knew the action of light upon silver salts, and tried to make pictures, but failed. It was not done till about thirty years afterwards, and not well done on paper for fifty years.

Even in those things demanded in daily life how slow is thought, and how confined to a few! Phosphorus is known for two hundred years before we can make matches by its help. Ether is known, we do not remember how long, but very long before it can be used for preventing pain in some of our greatest days of trial. Davy tried the inhalation of gases, and he, a man of genius, missed anæsthetics. We have here the same great fact—it is individuals who are remarkable, and few individuals. It is so in size, it is only occasionally that one man is larger than his fellows. It is so with the lower animals also, and one horse, or one cow, bull, sheep, or swine, when thus advanced by nature, becomes so much admired, that its race is soon carefully spread over all the country, at least as much as time will permit,—an artificial added to a natural selection. It is the same with plants, and one rapidly spreads its progeny everywhere, advertised in all corners and sold by all florists.

Perhaps some will say that rarity is true of great inventions and not of small. The greater the invention, the greater certainly is the truth of that for which we argue. The smaller the invention is, the greater is the chance of many persons making it. That is admitted. And surely there ought to be some limit to the admission of an invention into the patent list. There is a certain class of trivial ideas that ought never to be admitted in our opinion, and there is a class of dreamy patents which may be called nonsense. A man ought to make his idea clear, otherwise it is a speculation at best and unfit for the arts. Then there is the whole class of subsidiary patents, such as are mere branches of others. These seem to us the only class difficult to deal with, but we trust in the wisdom of our lawyers and scientific men to overcome these difficulties.

Another point has been lately raised, shall we give patents to those who bring inventions from abroad? At first, one is inclined to say, let the patents come from any part so long as the community is benefitted. On the other hand, it may be said, that a person who sees an invention in a finished state abroad has really little trouble in introducing it here, and cannot stand in the place of the real inventor. The rule we believe is, that if it exists in a foreign country unpatented for a few months it cannot be patented here. We should make quite the contrary arrangement. If it existed for a long time in a foreign country patented or not patented, if it were only useful, we should thank the man who brought it. If it were a thousand years old and no man during all that time had sense to bring it to us, we should reward him who at last saw its advantage. His vision is his genius.

Yet we confess the two classes stand on different bases, but we prefer to treat them equally, and imitate, as a nation, the abundant liberality of some of the Saracenic and Eastern princes, who are said to have had beside them the wisest and best men that could be found; each receiving abundant reward for the importation of his wisdom and the example of his goodness. The age haggles, splits hairs, reasons small, and imagines itself to be sharp, but broad, fine, human feeling, will bear down all this little appearance of exactness. Who cares if every blade of grass on the lawn has the same shape if only the whole lie smooth and equal. A few great principles laid down would get rid of much of the small reasoning and law-making. To obtain a patent a man must declare that he is the first inventor. This test act ought to be abolished. It seems as if few men knew who first invented anything. It is quite sufficient for a man to say that he believes he has a right legal and moral to apply for a patent, and that no man has a right to oppose him. This would also allow a man to have his patent taken out by another man chosen by him. What is it to the community whether the real inventor's name is given or not? We say again, the inventor would prefer, as a rule, to hide his name, because money is the object of inventors in the useful arts. A patent is a matter of business. If an inventor wants fame, he seeks it by publishing his inventions in journals or scientific societies. This of itself is a sufficient answer to those men who insist on saying that inventions would be as numerous without patents as with.

It has been proposed that no questions shall be asked in taking a patent—that is, that every man shall patent what he pleases. This looks quite a patent agent's idea to increase business. We think it a mistake. It will help many men to impose on the public. They are too much helped already, and men with impunity even use the word patent when there is no patent. It is a confusion of right and wrong, and this is an abundant reason for refusing such a system. It is in reality practised now but not avowed. We object to the practice and the avowal. Let a patent be a real invention, with truth on the side of both the contracting parties—the patentee and the government.

A proposal has been made to extend the duration of a patent to twenty-one years. With our opinions we cannot object to this. We know by abundant observation that it is difficult in the space of fourteen years to bring a patent, even an excellent one, into use, and we know, also, that expenses must be incurred during the whole time. This is very contrary indeed to the supposition of some men who imagine that an invention is an idea caught by accident and given to the public without expense. Men labour for the whole fourteen years, not only endeavouring to perfect the manufacture, but to teach the public its advantages. If fashion should suddenly desire a mauve or magenta colour, the lucky manufacturer gains money rapidly, because fashion has a method of teaching the whole Christian world its ideas, principles, and practice in a few months, and thoroughly outstrips all societies by whatever name for the improvement of mankind in everything whatever. But let it be shown that certain kinds of water must not be drunk at all, and that others require at least to be filtered, ages seem insufficient to teach this, and even a plague can do it but partially. The inventor who was assisted by fashion would make his money at once; the man who would make a perfect filter, supposing such a thing to be found, would probably struggle for years. It would seem very unfair to leave the latter to be robbed as soon as he should have by great expense taught the public, and it does not seem fair to treat the two differently. Besides, if fashion may treat one well for a while, it may leave him with equal suddenness. To our mind, the very least additional benefit that could be granted to patentees would be a readier mode of obtaining an extension of the time. The enquiries regarding extension of patent right do not satisfy. It is extremely difficult to get the truth fully told, and science has but little to say in courts of law, or says it with a small subdued voice easily silenced, because it has no recognised legal position —a mere tool tossed between good and bad, instead of a great and benevolent power, more than national, more than world-wide. Perhaps its cultivators are chiefly to blame; who can tell? An objection to patents is often raised by those whom we may call cosmopolitans. Their ideas are alluring; they lead us to large and unselfish thoughts, and when we think of "the great world spinning for ever down the ringing grooves of change," our small every-day duties become invisible,—we almost imagine that they may as well be left undone, and then we come to a sudden stop. If we all thought so, of what character would be the changes in the world; they might be backward for man,—it is only by minute attention to details that great establishments are kept in order. This is enough; the man who works for his fellow-creatures over all the world with equal ardour will show a small result of his labours; experience shows that he will do more by cultivating more carefully a smaller portion around him to make it an example and a power. No men have done more good to the world than patriots whose aims have been pure and their views confined entirely to their own country. The reason is simple enough. Man is small and his life short, and he does best who does his work most perfectly; if perfectly, it is necessarily in a confined sphere. There are a few men whose office seems more brilliant—men who bind together the various attempts of individuals, communities, and nations; but this is only possible when the work at home is well done.

We have little more to add, and we have said nothing that had not in spirit been said elsewhere; but as desired, we thought on the subject for a little, knowing that it occupies the minds of many persons, although it has come before the public very rarely as yet, and the public do not seem to know how much it concerns them. On one point only we shall add a proposal, namely, expense. We know from observation that the payments are felt to be very heavy by many deserving and industrious inventors, and if Government will not grant a diminution, it may be, perhaps, persuaded to allow these payments, or at least one of them, to be made somewhat later than now. Probably the best way would be to take the payments, with the exception of a very small preliminary one, out of the profits of a patent, to be collected as the income-tax is. A good patent might very well pay a considerable percentage. We calculate, or at least we conjecture, that 1 per cent on gains by patents cannot be less at this moment than £20,000; and it would be no hardship to pay 2 per cent, besides the ordinary income-tax of course. This would be enough for all that is required for registration, examination, and publication, and there would be a pleasure in seeing payments made by men who had prospered, instead of by poor men who had nothing but hope before them.

We may sum up the principal points:—

1. A man's property in his own invention ought to be protected as carefully as his property in land, or in houses, or in any movable goods.

2. As an invention when once explained is more readily taken from a man than houses or lands, special protection ought to be given, just as exposed property receives more of the attention of the police.

3. As inventions in the arts are made for the purpose in almost every case of making money, it is not good to burden them by exactions before the money is made. The true principle would be to make the gains of the patent pay, not all equally as now, but in proportion to the gains. This would be paid with pleasure, and the Government would receive the payment with pleasure, because it had given an equivalent by affording the patentee special protection.

4. There ought to be some discrimination in giving patents. Even a little advice to persons might be useful. When they come with their little fancies they might be shown that it would be better to go home without a patent.

5. A better mode of judging patent cases is required. The scientific element has not its due position. It would not be good to give it all the power. Scientific men have faults like other men, and one is that they are apt to be afraid of or to avoid ideas new to science, although these

new things may be the most rare and desirable. This preference for the general supposed course of true science sometimes blinds them.

6. Although we think this the age of inventions, we are in every corner in want of more. It is for the good of the nation and the world that they should be produced more rapidly. They would be if encouragement were given; the better kind would probably soon supersede the lower. It is proposed that every obstruction now depressing the minds of inventors, and keeping back some of the best, shall be removed, and an attempt be made to put in its place an encouragement or facility.

VI. ON MODERN BRITISH ORDNANCE AND AMMUNITION.

By Lieut. S. P. **Oliver**, Royal Artillery.

IN the last number of the "Quarterly Journal of Science," the new 11·6-inch gun* of 35 tons was alluded to as the latest addition to the British Service ordnance. The following details of its dimensions and construction are given beneath, viz.†:—

		Inches.
Nominal length		191·5
Total length		195 = 16 ft. 3 in.
Trunnions	Width between	56
	Diameter	13
	Width	7·75
	Axis from breech end	73·15
Length of	Bore	162·5
	Rifling	135
Twist increasing from 0 to $\frac{1}{35}$ calibres		406
Grooves 9	Width	1·5
	Depth	0·2

Vent: hardened copper placed at an an angle of 45° with the vertical, and enters the bore at 12 inches from bottom of bore, so as to strike a battering charge of 120 lbs. pebble powder at 4-10ths of its length from the end of cartridge.‡

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* This gun has since been bored up to 12 inches, with at present doubtful advantage.

† *Ante*, Minute 28,806.

‡ See Proceedings of Department of the Director of Artillery, Minute 28,879.

CONSTRUCTION.

			Inches.
Steel tube	Length		168·5
	Thickness at	Muzzle	2·45
		Breech	3·45
Breech piece (coiled)	Length		104·5
	Diameter (exterior)		34·5
1 B. Coil, length			57·5
B. Tube, length			56·5
C. Coil	Length		90
	Diameter (exterior)		56

Palliser shell { Length 33 inches.
Weight 700 lbs.
Minimum Service charge, 75 lbs. pebble powder.
Maximum battering charge, 95 lbs. ,,
Intended for Turrets of "Devastation" class.

With a proof charge of 130 lbs. of powder, *i.e.*, nearly half as much again as the Service battering charge, the 700-pounder projectile left the muzzle with a velocity of 1370 feet per second; the copper piston in the rear of the shot, by which the pressure of the gas in the interior of the bore is estimated (according to the depth to which it is driven in upon itself), indicating a strain of sixty-four tons to the square inch.*

This is certainly the finest gun ever turned out by the Royal Gun Factory, to which it does so much credit, and is a signal victory of the F. or cheap construction. It is greatly to be doubted if the famous 1000-pounder of 50 tons, constructed (of cast-steel subsequently densified by forging), by Herr Krupp, at his Titanic Essen establishment, would stand such a crucial test as that to which the 35-ton Fraser has been so successfully submitted.

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* The Special Committee on Gunpowder and other Explosives, in investigating the action of gunpowder, &c., in the gun, make use of three methods:—1. The determination of the time a projectile takes to traverse various intervals within the bore of a gun, which is accomplished by means of Captain Noble's chronoscope, which registers by means of electric currents upon a recording surface (revolving discs), travelling at a uniform and very high speed, the precise instant at which a shot passes certain defined points in the bore. 2. The velocity outside the bore at the distance of fifty yards from the muzzle, by the Navez-Lenes' chronoscope. 3. The "*crusher*" gauge, which is alluded to above, which determines the pressure exerted by the gas at various points of the bore (usually five in number). If the "*crusher*" be inserted in the *breech* the cylinder is of copper; if in the *chase* of the gun where the pressure exerted is comparatively small, it is of an alloy of *lead*.

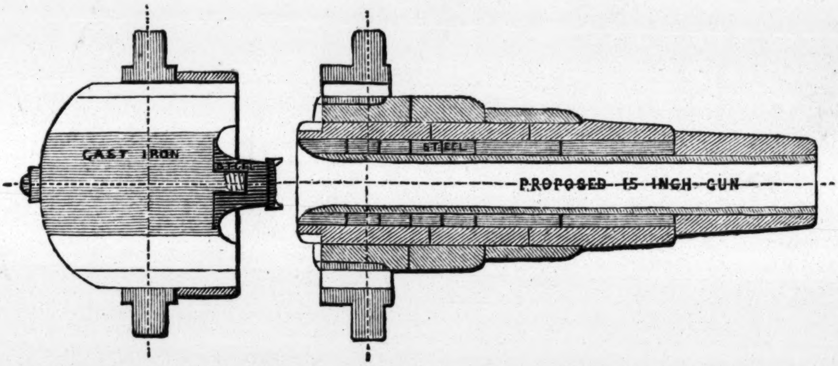
With a piston area of 1-6th of square inch, a pressure of 20 tons on the square inch reduces the length of a *copper* cylinder from 0·500 inch to 0·229 inch; similarly a pressure of three tons on the square inch reduces a *lead* cylinder from 0·500 inch to 0·240 inch. This "*crusher*" has been made so as to fit the bush in the base of all Palliser shells. Much better results have been furnished by the "crusher" gauge than by the Rodman system.

The observations for pressure and velocity independently taken by the three above-named methods, have corroborated and confirmed one another in a remarkable degree. For details see Proceedings of Department of Director of Artillery; Minutes 27,929, 28,072, 28,738.

The last startling sensation in propositions of new guns has been made by Captain Morgan, Royal Artillery, in his "Proposal for a *very heavy* breech-loading gun of novel construction."

This novel arrangement is certainly original, and consists in the gun being formed of a barrel with a movable and extraordinarily heavy breech, which is to be blown to the rear by the explosion; in other words, the gun is to fire at both ends, the force of the discharge partly expending itself upon the heavy breech-piece, which is to recede up an inclined plane. A model was exhibited lately at the Royal Artillery Institution, on a scale calculated for a 15-inch gun, firing 200 lbs. of powder with a projectile of 1200 lbs. weight.

FIG. 14.



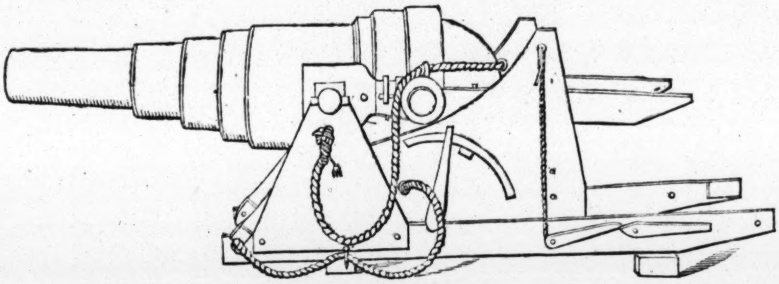
Captain Morgan's Proposed Monster 15-inch Breech-Loading Gun. Charge 200 lbs. of Powder. Weight of Projectile, 1200 lbs. (From Minutes of Proceedings of the Royal Artillery Institution, vol. vii., p. 145.

Captain Morgan is perhaps too sanguine when he states that he is "confident that on his system a 20-inch gun might be obtained firing a 1-ton shot, with a breech-piece and barrel of 50 tons each." If foreign naval constructors commence putting 20-inch iron plating on their armour-clads, we shall need 20-inch guns to pierce them.

Outsiders appear to have very vague ideas of the time in which such heavy guns as we now have in the Service can be loaded, run up, laid with accuracy, and fired; and perhaps the best way of affording information on this point will be to quote the results of actual experiments for rate of firing made at Shoeburyness.

By this means it has been ascertained by practice, that a 9-inch M. L. rifled gun of 12-tons, mounted on a wrought-iron carriage, with casemate slide, and traversed by means of tackle, can be worked by a detachment of thirteen men under a N. C. officer, loaded by a 30-pound cartridge and 250-pound plugged shell, laid on a target (5 feet square and moving at the rate of 8 miles an hour), at a thousand yards range, and five rounds fired in three minutes and twenty-two seconds. The moving target being hit at least once in the five rounds; whilst every shell will be thrown so near the target that it would hit a mark as large as a man-of-war launch.

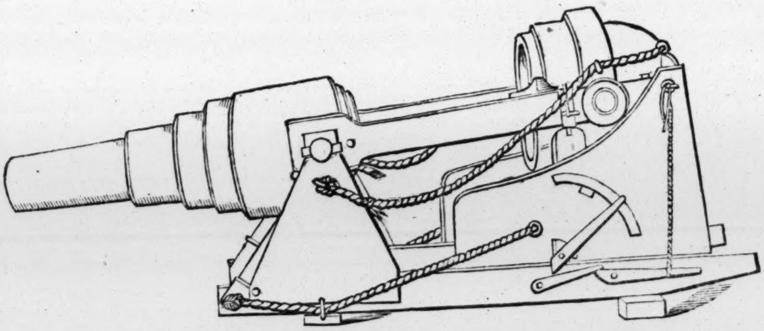
FIG. 15.



Loaded ready for Firing

So also at a standing target, the 23-ton gun, 12-inch, or 600-pounder, has been fired a series of rounds through a port with an average rate of one minute and thirty seconds for each round. In the instance quoted the above-named gun was manned by a detachment of seventeen men; it was mounted on a turntable, which had to be traversed round each time to bring the muzzle under the cheeks of a 16-foot gyn, rigged as shears, in order to enable

FIG. 16.



After Discharge, showing recoil of Breech-piece.

the 600-pound shell to be lifted up and placed in the bore. In another instance the same gun was fired three times in four minutes and nineteen seconds. Compare this with the ancient rate of firing heavy cannon two hundred years ago, as given by William Eldred, sometime master gunner of Dover Castle, during the reigns of Queen Elizabeth, James I., and Charles I. "One may well make ten shots an houre if the peeces be well fortifyed and strong; but if they be but ordinary peeces, then eight is enough, always provided that after forty shots you refresh and cool the peece, and let her reste an houre, for fear lest eighty shots shall break the peece, being not able to endure the force and heat."* The largest piece of British ordnance at that period (1646), appears to have been the Canon Royall, weighing 71·5 cwts., and throwing a 63-pound shot.

From what I have mentioned above, therefore, it would appear that our modern ordnance, although apparently cumbersome, are very manageable, and much more handy than the unprofessional observer would at first suppose.

Captain Sharpe's ingenious models of "*revolving guns*," now exhibited at the International Exhibition, Kensington, cannot be passed by without the notice which they deserve; their economy of space is great, and the amount of dead weight reduced to a minimum, whilst their extraordinary facility for obtaining extreme angles of elevation is a prominent feature of the system. Mr. Watts thinks the system admirably adapted for gunboats, and two great naval authorities, Sir Thomas Hastings and Sir Robert Smart, both agree in praising it. At the same time I must demur to the following paragraph in Commander Sharpe's pamphlet:†—

"Our present 25-ton guns are but carronades on a large scale, with a smashing rather than penetrating power, and have not even the proportionate length of the old 32-pounders of 56 cwts., which is about 19 diameters of bore in length. Following the same proportion, the 25-ton gun, with its 12-inch diameter bore, ought to be 19 feet in length, whereas it is little more than 14 feet" (p. 17). And again (p. 18),

"The tendency of modern gunnery is to give wide diameters of bore with low velocities to the shot—a practice the very reverse of what the penetration of iron-plating plainly requires. A velocity of 1600 feet in a second, on which the old Woolwich ranges were estimated, is now rarely if ever gained; the 25-ton and other similar guns are *therefore relatively far less powerful than guns of twenty years back, possessing neither the relative range nor relative penetrating power of the old 32-pounder of 56 cwts.*"

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* Proceedings of the Royal Artillery Institution, vol. vi., p. 283. On the Field Artillery of the Great Rebellion. By Lieut. Hime, R.A.

† A Description of Revolving Guns, Expanding Carriages, and Winged Shot. By Commander Sharpe, R.N. 1871.

Now, according to Captain Noble and Captain Stoney, gun-power is* properly estimated by calculating the dynamical force (*vis-viva*, or energy) of the projectile at various ranges, in foot-tons; whilst the comparative penetrative effect by dividing the *vis-viva* by the diameter of the shot; from this it is found that the 12-inch gun can deliver a blow at 1000 yards range twelve times as severe as the 68-pounder smooth-bore (95 cwts.), whilst as regards their relative power of perforation, the 12-inch gun is eight times as powerful as the 68-pounder.

By recent experiments it was found that our 10-inch guns of 18 tons penetrated 15 inches of iron (in three 5-inch plates), upon which the 15-inch Rodman smooth-bore American gun of 20 tons only made a shallow indent.

Captain Sharpe's expanding carriage is ingenious, but not to be compared with Captain Moncrieff's, whilst it seems doubtful if his proposed winged-shot are theoretically or practically possessed of the advantages claimed for them; indeed, the inventor would appear to be desirous of giving up the rifling principle and returning to the old smoothbores, when he says, p. 23, "Striking the wings obliquely gives a rotary motion to the shot (*should such motion be required*)."

Our present service Palliser-shells at present meet nearly all practical requirements, and when fired with full battery charges at 200 yards range, are calculated to perforate iron plates of a thickness of 1 inch greater than their diameter.† A few facts about these shell may not be uninteresting; first, as to their shape, which is cylindro-conoidal. It has been found that for accurate shooting a length of at least two calibres is necessary, but of course this varies immensely; thus the double shell of a 7-inch gun is 27·2 inches, *i.e.*, all but four calibres; after many experiments as to the best form for the head of the projectile, it has been decided nearly conclusively that an ogival head struck with a radius of 1½ diameters is the best adapted both for flight and penetration. It has also been found that shot fly better when hollow than when solid, the weight when distributed further from the axis giving a longer radius of gyration.

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* Royal Artillery Institution Paper, vol. vi., p. 116 *et seq.*

† See Captain Orde Browne's Treatise on Ammunition for Rifled Ordnance, p. 215.

These principles govern their form. Next as to their construction. The so-called chilled Palliser-shot or shell are cast of a peculiar iron in sand-moulds, with metal ends or chills for the heads; the head of the projectile is thus chilled white and rendered intensely hard, fit for punching a hole in an iron plate, although at the same time it is so brittle that the tip or point is occasionally broken off by the impact of a shell rolled or struck obliquely against it; for, strange as it may appear, the point which may penetrate directly through several inches of armour without injury may be fractured by a very slight transverse blow (Captain Orde Browne). On the other hand, the body of the shell (or rather sides and base, for it is cast with a core) being cast in sand, is partially annealed, and composed of an evenly mottled iron possessing more tenacity and toughness, which enables it to better withstand the shock of discharge. All these shells are cast with undercut holes, into which the rifling studs of a soft alloy (10 parts copper to 1 tin) are swedged.

Our iron-clad Navy and our plated Forts (when they are plated) need have little to fear as to the result of an artillery duel, whether they have to meet 1000-pounder Krupp's, or the 20-inch Beelzebubs and Puritans of the American turret-ships. The English public may assure itself what all foreign powers will be ready to admit, that we possess the most powerful ordnance and most unrivalled ammunition in the world, and that we owe our possession of them to the ability of our departmental officers. We owe our present construction of guns to Colonel Campbell and Sir W. Armstrong, with Messrs. Fraser and Anderson; the carriages on which they are mounted and accessory machinery to Colonel Clerk and Captain Moncrieff; our powder to Colonel Younghusband; our projectiles and exquisite fuzes to Colonel Boxer and Mr. Abel; our chilled shell to Major Palliser; with many others, hard-workers and able coadjutors in every department. Woolwich is well up to, if not in advance of, the age, and her factories can produce, if necessary, 6000 tons' weight of wrought-iron cannon per annum. Already there is a talk of turning out larger guns, and the drawings of 800-pounders, and even 1000-pounders, are actually ready, and can be manufactured whenever the Secretary of State for War thinks fit to order them, so Captain Stoney informs us,* and, moreover, adds that the time necessary for their manufacture is at the rate of one week per inch of calibre; for example, twelve weeks for a 12-inch gun.

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* Proceedings of Royal Artillery Institution, vol. vi., p. 432.

NOTICES OF BOOKS.

The Beginning: its When and its How. By MUNGO PONTON, F.R.S.E. London: Longmans, Green, and Co. 1871. Pp. 572.

THIS may fairly be described as an extraordinary work. Touching upon upwards of a thousand topics, laying under contribution almost every member of the circle of the sciences, obtaining aid from philological research, and entering, sometimes deeply, into the mazes of metaphysics, it induces upon the reader a feeling of almost perplexity, which is only overcome by the extreme interest which it inspires. The author divides his work into two parts. In the first part, the teachings of the Sciences are laboriously examined with the object of obtaining the most probable notions concerning the antiquity and the conditions of matter, the origin of suns and planetary systems, the luminiferous ether, the evolution of vitality, and the origination of genera and species. In the second part, these teachings of science are placed parallel with the biblical records; and the conclusion is drawn, that faith and science can go hand in hand; that true philosophy and religion, far from offering violence the one to the other are, when viewed in the true spirit of scientific enquiry, mutually corroborative.

After an ingenious argument, reducing to philosophical certainty the view that the luminiferous ether must be infinite in extent, the author, in regard to the question of the antiquity of matter, considers that the probabilities that matter existed from all eternity, and that it was created by the volition of an eternal mind, are nearly equally balanced. Obviously as the human mind can, from the very nature of things, form no conception of matter apart from force, every dictum concerning it abstractedly must be pure speculation; but the author considers that matter which existed in space was at first an "assemblage of substantial ultimates, each having definite size, definite form and impenetrability, but having no relative properties whatever, each ultimate being absolutely indifferent to every other ultimate in the universe." The first species of physical force was probably the endowment of these ultimates with mutual repulsion; hence the first conception of ether. The next step was the application of the divine energy to produce a vibratory motion upon the ultimates, those constituting the luminiferous ether and those which afterwards became ponderable being at first probably indistinguishable. Thus the same vibration which in the case of the imponderable ultimates constituted light, in the case of those subsequently endowed with gravity constituted heat. The laws of gravity could of necessity be alone the work of an infinitely intelligent and calculating mind. From these commencements followed the assemblage of the substantial ultimates into separate centres, and the evolution and co-ordination of the various forms of force. The earth, which was probably at its earliest stage gaseous, condensed somewhat suddenly into liquidity or viscidity, the temperature of space being probably very high. The cooling and the formation of the crust of the globe was probably very protracted, the atmosphere was constituted by the escape of vapours through fissures in the crust, volcanic action producing the irregularity of the earth's surface. After a rather short chapter on the moon, the author enters at length upon the question of the probable constitution of the sun, excluding the view that it is an incandescent mass or a body in a state of combustion. Its bright "faculæ" are not flames, but may be produced by the heaping up of (hypothetical) light-generators into masses of greater thickness than the general photosphere. The solar surface is acknowledged to be highly electric, but objections are raised to the view that the sun's light is due to electric discharges passing through highly rarefied and very dry gaseous media. The "meteoric theory" is met also by many objections. The force generated in each cubic foot of the solar photosphere would probably be equivalent to that sufficient to raise 5½ lbs. a foot high in a second. This energy is probably not greater than that which many living beings are capable of displaying as mechanical force or as luminous or thermal vibrations. It is obvious that the author inclines to a belief in special solar light-generators, the nature of which is undiscovered. The author considers the primeval vegetation of the earth to have existed before the sun; it was probably destroyed to afford the pabulum which was necessary for subsequently appearing forms of animal life, centralisation of light in the sun intervening between the two epochs. The author illustrates his description of the low forms of organisation by some beautiful drawings of *Foraminifera*, *Polycystina*, *Diatoms*, the spicules of sponges, &c., his object being to show the marvellous evidences of superintending design afforded by the beauty of the productions of these organisms. After showing the appearances of living things to be not simultaneous but successive, the author enters upon the question of vital origination. The hypothesis of "spontaneous generation," or, as he terms it, "apparent organic origination," finds in him a strong opponent. The champions of this theory can scarcely be gratified with the manner in which their views have been received by the cultivators of science. M. Pouchet found that a majority of the *savans* of the Académie des Sciences condemned his conclusions. Dr. Bastian, who fights the battle in this country, found in Professors Huxley, Tyndall, and Frankland, prophets who blessed the enemy. And Mr. Ponton considers that all experiments fall short of proving the production of living organisms from dead organic materials in conjunction with the ordinary forms of physical force.

He commences with a wide and somewhat unsatisfactory definition of life as "an energy capable of influencing both the material ultimates and the physical forces." There is in man a conscious revelation that life consists in a somewhat distinct from the elements of his organism; this somewhat Mr. Ponton terms an *organiser*, which receives its organising faculty direct from the Creator. The organiser is characterised by four properties—indefinite extensibility, indefinite divisibility, penetrability, and the capability of binary combination; it cannot be material, but itself exercises a peculiar influence over matter; it is not itself a force, but it has a special power over the physical forces, in virtue of which it can compel the ultimates to perform motions altogether unlike those which they perform under the influence of the physical forces alone. The organisers, according to our experience, do not originate forces, but merely avail themselves of the forces already existing; but they are endowed, besides their faculty of organisation, with the psychical attributes of volition, instinct, consciousness, &c. The author devotes a chapter to "Protoplasm," and analyses the writings of Huxley, Stirling, and Beale. He strongly condemns the physical theory of vitality, which he interprets Professor Huxley to adopt, but is in very considerable accord with Professor Beale, who has devoted to the subject such laborious investigation.

Reverting to primordial creation, Mr. Ponton considers that the balance of evidence is vastly in favour of the view, that the earliest organisms were formed by a *gradual* process. Concerning their mode of evolution there are two hypotheses: the first enunciating that evolution of the various forms took place from a single organiser; the second, that during the creative epochs, the Deity established in succession a vast number of organisers. In considering the first hypothesis—that of Organic Derivation—the author adversely criticises Mr. Darwin's views. Whilst acknowledging the existence of certain difficulties, he leans to the second hypothesis—that of Specific Creation.

The second part of the work is of great interest. The author carefully analyses the expressions in the Scriptural account of the Creation in the original Hebrew, and places them side by side with the hypotheses deduced from scientific observation; the lessons derived from these mutual interpretations are in some instances startling, and, though in some they are in the nature of vague surmise, the author succeeds in impressing his reader with the conviction that the two records, observed and revealed, have very close mutual co-aptations. In every part of his work the author has placed himself *en rapport* with the actual state of science, and his observations will be read with interest by a large number of scientific and literary men.

The Manufacture of Russian Sheet-Iron. By JOHN PERCY, M.D., F.R.S., Lecturer on Metallurgy at the Royal School of Mines, and to the Advanced Class of Artillery Officers at the Royal Artillery Institution, Woolwich. London: John Murray. 1871.

THE method of manufacturing Russian sheet-iron has long been thought a process the details of which were supposed to be kept secret by the various firms interested in the production. But Dr. Percy has succeeded in presenting to the public a full account of the process afforded him by several eminent Russian engineers employed in the manufacture. This particular variety of sheet-iron differs from that produced elsewhere in being remarkable for its dark grey, polished surface, and its extreme ductility. It is extensively used in Russia for roofing, and in the United States, where it is known as stove-pipe iron, in the construction of locomotive engines and for stoves. The pig-iron employed in the manufacture results from the smelting of magnetite and the red and brown hæmatite ores with charcoal in a cold-blast furnace, the conversion of the pig-iron into malleable iron being effected either in the charcoal finery or by puddling. The malleable iron is rolled and beaten into bars and sheets, and is then subjected to a re-heating process to which the ductile nature and peculiar colour are due. Packages of three sheets are heated to redness, and immediately before rolling there is strewn between the sheets a quantity of powdered charcoal. The rolled sheets are sheared to size, and bound up in packets of about one hundred sheets each. These packets are re-heated in a peculiar furnace, powdered charcoal having again been placed between each sheet; the packets are then thoroughly hammered. When the sheets are cool, they can be delivered to the purchaser. Dr. Percy suggests that, if an attempt should be made to extend the manufacture to this country, it would not be necessary to imitate the Russian process in every particular; and that instead of the peculiar annealing furnace used in Russia, the method of annealing in covered cast-iron vessels pursued in tin-plate works might be advantageously adopted. Dr. Percy is to be highly commended for this careful compilation on so important a subject, hitherto a matter of much doubt and controversy; and his work should be consulted for the chemical investigations as to the nature of the iron, and for the description of the peculiar machinery employed in its manufacture.

Power in Motion: Horse Power, Wheel Gearing, Driving Bands, and Angular Forces. By JAMES ARMOUR, C.E. London: Lockwood and Co. 1871.

To meet the wants of practical men engaged in engineering works requiring the use of tackle, driving-bands, wheel gearing,

&c., these manuals by Mr. Armour are admirably adapted. This member of the series is fully worthy of its predecessors, both in the fulness of detail, and in the simplicity of the formulæ employed. Most of the questions are solved by simple arithmetic and by logarithms; in cases where trigonometrical relations have to be considered, as in the determinations of angular force, everything is so clearly explained that no difficulty can be said to exist. There can be no doubt that Mr. Armour's works will be much appreciated.

The Elements of Plane and Solid Geometry. By H. W. WATSON, M.A., Sometime Fellow of Trinity College, Cambridge; late Assistant Master of Harrow School. London: Longmans, Green, and Co. 1871. Small 8vo., 285 pp.

THIS little work is one of a series of text-books of science published by Messrs. Longmans, and intended for the use of artisans and of students in public and other schools. Mr. Watson has with liberal views exposed himself to criticism in endeavouring to sift from the books of Euclid only the propositions and so much of each demonstration as will be likely to benefit those for whom he writes. It needs, however, only a glance at his work to show that a new era has dawned upon elementary geometrical science, when men of Mr. Watson's mathematical standing acknowledge our elementary teaching to be at fault. Euclid's prolixity—so puzzling to the young geometrician—has in this work given way to an extended application of the principle of superposition. It is true that Euclid himself recognises the importance of this principle, but he employs it timidly, and, as in Prop. 5, Book 1, often puts himself out of the way to avoid its use where it would greatly assist the student, and would, indeed, be the more correct method. Another innovation on which Mr. Watson is to be complimented, is the arithmetical treatment of ratio and proportion, thus rendering the 5th Book of immense value in instruction. There is, under the present system, hardly a boy of a lower form who, although having passed through fractions, has an adequate idea of the properties of ratio and proportion, while even in published scientific works of an elementary character, the terms are often confounded. The work certainly deserves the earnest attention of all who wish to see geometry studied elementally on a truly logical method.

A Treatise upon Terrestrial Magnetism. Edinburgh and London: William Blackwood and Sons. 1871.

THE writer of this work, which is unsigned, first considers the present theories accounting for the phenomena of the earth's magnetism, and then expounds a new hypothesis. Assuming the earth an immense magnet, and inferring from his reasoning that the sun does not influence the earth by acting upon it simply as a magnet, he then considers the only other known way in which the sun can produce terrestrial magnetic effects—namely, by bringing into existence a current of electricity. It is then argued that, assuming there is a mutual attraction between electricity and matter, and that therefore the matter of the sun attracts the electricity of the earth and *vice versa*, there would be a greater constant electrical density existing on the surface of the earth immediately under the sun. This is, of course, allowing the assumption that electricity is more or less free to move among the particles of matter of which the earth is composed, and that in a body moving through strata of electricity of varying density, the same effects are produced as when a current is caused to flow round the body. Now, as the earth moves from west to east, there will, on these assumptions, be a current circling from east to west, inducing a magnetic polarity north and south. The magnetic poles would therefore be coincident with the poles of the ecliptic, and would revolve around the terrestrial poles in the same time. But this is not the case, and the difficulty is surmounted by supposing that the earth is analogous to a magnetised bar of hard iron, and that, consequently, the changes in the magnetism are correspondingly slower in proportion to the hardness of the ferruginous matter and the strength of the current. The secular, annual, and diurnal variations now become easy of illustration, because they may be referred to so many combined causes.

Of the relation of sun-spots to magnetic disturbances, the writer remarks:—" Of the nature of this connection we have at the present time no knowledge. It may be that this connection is not direct, but results from an intermediate agency—viz., that of the aurora; for there appears to be a like variation in the auroral displays, these being more frequent in proportion as the number of sun-spots are greater, and *vice versa*. The aurora, when present, always disturbs the magnetic needle; the greater or less amount of disturbance may, therefore, be dependent upon the greater or less frequency of auroral exhibitions."

The reasoning throughout is ingenious, but founded on so hypothetical a basis, that it is clearly impossible to draw conclusions. The arguments in favour of the theory, adduced in the shape of calculated charts as compared with recorded observations, are very voluminous and certainly striking. The book is well worth perusal as a very fair and clear statement of what has been observed and advanced with regard to the magnetism of the earth. The collection of charts, copies from those of Sir Edward Sabine, would alone render the work of value.

A Handbook of Practical Telegraphy. By R. S. CULLEY, Member of the Institute of Civil Engineers, Engineer-in-Chief to the Post Office Telegraphs. Fifth Edition. London: Longmans and Co.

THAT another edition of Mr. Culley's work should be so soon on hand shows that it is in increasing demand as a professional text-book. This edition far surpasses the preceding in completeness. Much that before was left unexplained to the student is now considerably elaborated, and there has been a valuable addition of plates and descriptive matter relating to the connections of Sir C. Wheatstone's automatic system. Professor Hughes's type-printing telegraph is also more fully detailed. There is introduced an explanation of Sir W. Thompson's new syphon-recorder; and much necessary detail has been added on the subject of relay and translating systems. A relay is, perhaps, the instrument presenting the greatest difficulty to the learner, because the most complicated on an ordinary telegraph line. In submarine work especially, much has been done for the reader; the formulæ have been re-arranged, engravings introduced showing the connections of the Atlantic and other long cables, and secondary and cable currents very tersely explained. There are also many data and general remarks as to the management of circuits in this edition that will be valuable to the working engineer. It must be remembered that Mr. Culley has to write with a double purpose—to convey information to learners, and to provide a handbook for the gentlemen of his staff; and certainly he is to be congratulated on the happy result attending his endeavours. There is no doubt that the edition now submitted to the public is the most practical handbook in the profession; it is devoid of the unnecessary mathematical theorising that has unluckily beset telegraphy generally. The book will be very acceptable to those now studying for contingent appointments in the Indian Telegraphs, especially as it is the recognised text-book of the department.

A Complete Course of Problems in Practical Plane Geometry; adapted for the use of Students preparing for the Examinations conducted by the Science and Art Department. By JOHN WILLIAM PALLISER, Second Master, and Lecturer on Geometrical Drawing at the Leeds School of Art and Science, Mech. Inst., &c. London: Simpkin, Marshall, and Co. 1871.

THIS is a capital little work, giving the construction of the geometrical figures plainly and systematically. But there is one great fault—it has been too hastily edited, and numerous typographical errors have been allowed to pass; thus, the *radical sign of square root* in the definition is said to signify that the expression to which it relates should be *squared*. These minor errors greatly deteriorate a work that would otherwise be of the highest utility, not only to the student, but in every office where drawing has to be done.

Switches and Crossings. Formulæ for ascertaining the Angles of Crossings, the Lengths of Switches, and the Distances of the Points of the Crossings and Heels of the Switches from the Springing of the Curve. By WILLIAM DONALDSON, M.A., A.I.C.E., Author of a Treatise on Oblique Arches. London: E. and F. N. Spon. 1871.

IF plate layers, or even the foremen, for whom Mr. Donaldson professes to write, have to study works of this character, one can hardly wonder that something should go wrong somewhere. From beginning to end the book is a mass of inexplicable formulæ —inexplicable because ordinary mathematical expressions are made subservient to other renderings. Having his symbols to construct, surely Mr. Donaldson could have devised something better than the employment of the ordinary signs of the powers and the series. Formulæ for practical application should be so written that a mathematician of tolerable acquirements can read them at sight; but here one is perplexed by the subversion of conventional symbols, and has continually to turn to the first chapter for explanation. But, this notwithstanding, every credit must be given for the patient labour entailed by so much calculation. The idea is good, and all cases of junctions are discussed; but is all this labyrinth of signs and letters necessary? If so, there can never be a dearth of mathematicians as long as a platelayer is to be found.

Elementary Treatise on Natural Philosophy. By A. PRIVAT DESCHANEL, formerly Professor of Physics in the Lycée Louis-le-Grand; Translated and Edited, with extensive additions, by J. D. EVERETT, M.A., D.C.L., Professor of Natural Philosophy in the Queen's College, Belfast. In four parts: Part II. London: Blackie and Son. 1871. Medium 8vo.

THIS is the second volume of Professor Everett's translation of M. Deschanel's "Traité Elementaire de Physique," and is exclusively devoted to the consideration of Heat. Of the admirable character of the work we have already had occasion to speak when noticing Part I.; and this character is fully maintained. The subject is considered under the heads of Thermometry, Expansion of Solids, Liquids, and Gases, Fusion and Solidification, Evaporation and Condensation, Ebullition, &c. A very clearly written chapter on Thermo-dynamics, embodying the most recent experiments, the editor claims for himself. Professor Foster's nomenclature of units of heat, the gramme-degree, pound-degree, &c., has been adopted. Comparing the work with the French original, it is easy to see that great care has been bestowed upon its production, and that much fresh matter has been introduced.

The Technical Educator; An Encyclopœdia of Technical Education. Volume I. Cassell, Petter, and Galpin: London and New York.

THIS work is a practical sequel to the theoretical lessons contained in the "Popular Educator," published by the same firm. The main subject is Drawing, with its applications to Design and Ornamentation; but Civil and Military Engineering have a great deal of space devoted to them; while under a third head may be considered Practical Chemistry as applied to Manufactures and Agriculture. The several papers are written in a clear, concise manner, admirably adapted to convey the amount of information required by the artizan and mechanic for whom the pages are intended. Too much praise cannot be afforded to Messrs. Cassell for their many and successful attempts to place sound elementary knowledge within the reach of the working classes.

Annual of Scientific Discovery; or, Year-Book of Facts in Science and Art, for 1871. Edited by John TROWBRIDGE, B. Sc., Asst. Prof. of Physics in Harvard College; aided by W. R. NICHOLS, Asst. Prof. of Chemistry in Mass. Inst. of Technology; and C. R. CROSS, Graduate of the Institute. Boston: Gould and Lincoln. London: Trübner and Co. 349 pp.

THIS is a *resumé*, chiefly from the various periodicals, of the most important discoveries and improvements in science during the year 1870, including a list of recent scientific publications, and obituaries of eminent scientific men. As a reference for dates and main principles the work will doubtless be useful.

Astronomy Simplified for General Reading. By J. A. S. ROLLWYN. London: William Tegg.

THE general reader who is unlucky enough to take up this book with any amount of faith in its contents would be led to imagine that the most exact science was a myth. Mr. Rollwyn, who lives in a glass-house hypothesis of his own, is certainly to be pitied for throwing stones. When an author publishes a book intended to overthrow received scientific opinions, it is certainly, to say the least, unfair to our great scientific teachers, to dedicate his work to their implicit pupils, the vast mass of general readers. *Palmam qui meruit ferat.* First, then, why does not the author of this very mediæval work bring some of the discoveries in Spectrum Analysis, promised on the title-page, to bear upon his subject before he undertakes to render null the labour of those intellects who have annihilated space in the investigation of the planets. Bitter experience will teach Mr. Rollwyn that fact is to be met only by fact. But the hardest knock is left to the final chapter, where in a few lines we are treated to an easy method of squaring the circle. Again, does Mr. Rollwyn know that there is a form of argument commonly called begging the question. It is almost incalculable the amount of harm this work may do in the hands of those to whom it is addressed. With the exception that it is well got up, there is nothing to be said in its favour.

Transactions of the Woolhope Naturalists' Field Club for 1870. Hereford: The "Times" Office. 1871.

A MORE pleasant series of papers it would be difficult to find. Carefully edited, they are interesting, and in some instances valuable, descriptions of the natural history of places at which the members have met, the contributions on Fungi being well worthy of notice. The illustrations are good, especially the series of photographs of the remarkable trees of Herefordshire.

The Technical History of Commerce; or, Skilled Labour Applied to Production. By JOHN YEATS, LL.D., F.G.S., F.R.G.S., &c., assisted by several scientific gentlemen. London: Cassell, Petter, and Galpin. 1871. 431 pp. 8vo.

DR. YEATS, in bringing the second of his three volumes on Commerce before the public, expresses the hope that, as a sketch of Technical History, it will be found useful in assisting to direct the subject-matter of education more to the occupations of the people. And in this particular the work decidedly supplies the want of a manual clearly stating the progress of the various branches of industry, and to whom and to what we owe their origin. A quotation will show the neatness of style and arrangement. When treating of food-stuffs, Dr. Yeats says:—"Notwithstanding its great apparent variety, food, in its ultimate analysis, falls into four classes, viz.—proteids, fats, amyloids, and minerals. The proteids are compounds of carbon, hydrogen, oxygen, and nitrogen, and include such food-stuffs as the albumen of white of egg, the syntonin of muscle and flesh, the casein of milk, &c. The proteids subserve the process of the formation of tissue as their special function, though they share with the next two classes in maintaining the heat of the body. Fats contain but three of the four elementary principles mentioned above, nitrogen being absent. Their amount of hydrogen is more than sufficient to form water if combined with the oxygen of the compound. In this class are included all oils, fat of meat, &c. Amyloids resemble fats in composition, except that the amount of hydrogen is but just sufficient to form water with the contained oxygen. This class includes sugar, starch, dextrine, &c. Fats and amyloids perform the function of maintaining animal heat. The fourth class includes such substances as sulphur, phosphorus, lime, and the salts of various alkalies and earths. One of the chief functions of this class of food-principles is to afford strength to the more solid parts of the animal frame." More technical subjects are treated in a similar manner: the style is neither too difficult to be readily understood, nor so simple as to pall upon the reader. Dr. Yeats may be considered to have opened up a fresh path for our high-class school-book writers.

Handbook of British Fungi. By M. C. Cooke, M.A. 980 pp., 6 plates, 408 figures. Macmillan. 1871.

This long-expected work by one of our most industrious mycologists has at last made its appearance. It contains descriptions of all known species of British Fungi, amounting to the enormous number of 2809. Each genus is illustrated by a carefully executed woodcut. The student is aided in the discrimination of the genera of the *Agaricini* by a series of coloured tables. It is much to be regretted that the author has been unable to supply an introductory chapter on the structure, affinities, &c. of the Fungi, and in particular on the terminology employed; the work has, however, so far exceeded the limits originally intended that it has been found impossible to give such explanations however desirable. The author hopes to publish an introduction at some future time, and also occasional supplements. The work supplies a want felt by every mycological student, there having been no collected description of British Fungi since that by the Rev. M. J. Berkeley, M.A., in 1836; all information since that time had to be searched for in the pages of numerous British and foreign periodicals. The thanks of every lover of botanical science are due to the author for his valuable work, which though of necessity a compilation is not wanting in original matter.

PROGRESS IN SCIENCE.

MINING.

THE Royal Commission appointed some five years ago to inquire into the probable duration of our supply of coal has recently issued its long-expected Report. An enormous mass of information has been collected by the several committees into which the Commission was resolved, and after much painstaking investigation, some important general conclusions have been attained.

In attempting to fix a limit to the depth at which coal may be profitably worked, it has been found that the chief difficulty arises from the increased temperature consequent upon increased depth. With a view to determine the maximum temperature compatible with the healthful prosecution of mining work, some experiments were made by Dr. J. Burdon Sanderson in a Cornish mine, where the air was heated by a hot spring of 114½° F. It appears that the temperature at which work can conveniently be prosecuted depends greatly on the hygrometric state of the surrounding air. At the working face of the coal, air is always more or less humid, but the deepest collieries appear to be the driest. On the whole, the Commissioners conclude that a depth of 4000 feet may be fairly attained.

Another branch of inquiry related to waste in working coal. It appears that under a favourable system of working the loss is only about 10 per cent, but that in a large number of cases it amounts to as much as 40 per cent.

The estimation of the quantity of available coal in the known coal-fields of the British Isles was one of the main objects of the investigation. Taking 4000 feet as the maximum depth to which workings may be extended, and excluding all seams less than one foot in thickness, it is estimated that there exists in the several coal-fields of these islands, upwards of 90,207 millions of statute tons of coal. In addition, however, to this amount, it is well known to geologists that in certain districts vast tracts of coal lie hidden beneath the Permian, New Red, and more recent strata. Taking into account only such coal as will be found within 4000 feet of the surface, and deducting 40 per cent for loss and other contingencies, the Commission estimates that not less than 56,273 millions of tons of coal exist below these post-carboniferous rocks. Adding this quantity to that known to exist in our coal-fields, we obtain a grand aggregate of 146,480 millions of tons as the amount of available coal in the British Isles. It remains to consider how long this supply is likely to last. Assuming that our present rate of consumption—115 millions of tons per annum—remains constant, this amount of coal will not be exhausted for 1273 years. But such an assumption is obviously erroneous. Let the rate of production increase at the rate predicted by Professor Jevons, and this quantity will be exhausted 110 years hence. It is shown, however, by Mr. Price Williams, that although the population of the United Kingdom rapidly increases, yet the rate of increase is diminishing. Introducing this correction into Jevon's estimate, it is shown that the supply will hold out for 360 years. If instead of taking a geometrical increase in the rate of consumption, an arithmetical increase be calculated by adding 3 millions of tons each year, it is found that the supply will be exhausted in 276 years.

Another interesting question discussed in this report is the probable extension of the coal measures beneath the newer rocks of the South of England. Mr. Prestwich, following Mr. Godwin-Austen, argues in favour of this extension, but his views are strongly opposed by Sir Roderick Murchison.

The prosperity of some of our colonies is so closely connected with the development of their mineral wealth that colonial statistics of mining are always of interest. From Mr. Brough Smyth's "Mineral Statistics of Victoria for 1870" we learn that the quantity of gold exported from the colony in that year amounted to 1,222,798 ozs., being a decrease of 118,040 ozs. on the amount exported during the previous year. This diminution is certainly in part referable to the damage done by the heavy floods of 1870. The quantity of gold purchased by bank managers and gold buyers on the fields last year amounted to 718,727 ozs. from alluvial workings, and 585,576 ozs. from quartz veins. The total number of miners employed in working gold on the 31st December, 1870, was 60,365, and the average earnings per man for the year 1870 was £81 0s. 6d. From the first date of the discovery of the gold fields of Victoria up to the end of last year, the quantity of gold exported amounted to not less than 39,399,328 ozs. 6 dwts. Reckoning this quantity at £4 per oz., its value reaches £157,597,313. In addition to this, the quantity used in the colony should be added, but it is of course impossible to estimate this amount with anything approaching to accuracy.

Nearly thirty years' residence in Cumberland has given Mr. T. Ainsworth an opportunity of collecting "Facts developed by the Working of Hæmatite Ores in the Ulverstone and Whitehaven Districts from 1844 to 1871." These observations were presented to the British Association at the recent meeting. The author asserted that the hæmatite was not confined to the limestone, but might be found in other kinds of rock, and even between two different strata. He maintained that the distribution of the hæmatite bore some relation to the coal fields, and that the direction of the ore-deposits was tolerably constant.

At the same meeting a paper was read by Mr. J. Sinclair Holden on "The Aluminous Iron Ores of County Antrim." These ores have of late years acquired considerable economic importance, and are now extensively used for admixture with the siliceous hæmatites in order to flux the silica and produce an easy-flowing slag. Mr. Holden described the beds as intercalated among the basaltic rocks, dipping generally to the south-west, and being traceable along the coast for about seventy miles. A notice of these aluminous ores was presented some little time back to the Geological Society of London, by Mr. Ralph Tate and Dr. Holden.

At the annual meeting of the Miners' Association of Cornwall and Devon, Mr. Robert Blee read a paper on "The Comparative Health and Longevity of Cornish Miners." It appears that the rate of mortality among children of miners does not differ from that of other classes. Indeed, in the parishes of Camborne and Gwennap, the proportion of deaths among the children of miners was less than among other children, whilst in Redruth and Illogan it was somewhat greater. It was found that between the ages of 10 and 30 years, 28 per cent of miners died, as against 18 per cent of men who were not miners. Between 40 and 60 years of age, 36 per cent of miners died, and only 20 per cent of non-miners. 9·07 per cent of miners lived to the age of 70, whilst 31·06 per cent of men following other occupations reached the same age. Pulmonary disease was the common cause of death, and the means recommended for preventing its ravages were efficient ventilation of the mine, improved means of ascent, and protection from sudden changes of temperature.

The health of the Cornish miner also formed the subject of an address by Dr. Barham to the British Medical Association at their recent meeting at Plymouth. He contrasted the health of the miner in the West of England with that of the miners in Northumberland, Durham, and Staffordshire. In Cornwall deaths from consumption are immensely in excess of those in other districts, whilst the proportion of accidents is much less. The prevalence of consumption was referred to insufficiency of light and air, to exposure, and to liability to inflammatory affections.

A safety-lamp having for its especial object an increased power of illumination has been devised by Mr. R. Brown, of the Shotts Iron Company, Glasgow. A simple plano-convex lens is set in the lower part of the wire-gauze cage, and surrounded by a conical shell of tin plate, which serves at once to reflect the light and prevent breakage of the lens. The light is also increased by a reflector behind the flame. Further protection is afforded by the use of segmental shields, one of which is fixed behind the reflector, whilst two others are movable, and can be made to slide to a greater or less extent round the lamp.

METALLURGY.

Whilst it is well known that the Russians have long manufactured a thin form of sheet-iron having a beautifully smooth and black surface, the details of the manufacture have not generally been made public. Dr. Percy has recently collected several descriptions of the process, and has published a pamphlet upon the subject.*

The iron works at which this form of iron is produced are situated on the eastern slope of the Ural mountains. The furnaces are fed with magnetite, red and brown hæmatite, and carbonate of iron; these ores being smelted with charcoal. The puddled bars having been cut into certain lengths are heated to redness, and rolled into square sheets. These plates are cleaned with a wet broom of green fir leaves, powdered charcoal is spread between the sheets, and the plates, having been made up into packets of threes, are several times re-heated to redness and passed through the rolls. The sheets are then cut to an uniform size, and brushed over with a mixture of birch-charcoal powder and water. They are then arranged in packets of from 70 to 100 sheets each, placed in a re-heating chamber of peculiar construction, and the temperature slowly raised for several hours, whilst oxidation is prevented. The packets having been sufficiently heated are removed and placed under a tilt-hammer. Finished sheets are inserted alternately between the hammered plates, and the packets are then subjected to a second hammering, which removes the wavy appearance resulting from the previous hammering, and produces a smooth surface. The packets being then opened, the sheets are cleaned with a wet broom, and when cool are cut to the standard size, when they are ready for the market. The only secret of the operation by which the beautiful surface is produced, appears to consist in the use of charcoal powder.

At a recent meeting of the Institute of Mechanical Engineers at Middlesbro'-on-Tees, Mr. I. Lowthian Bell read an able paper, "On the Preliminary Treatment of the Materials used in the Manufacture of Pig-iron in the Cleveland District." The ore used contains 31 per cent of iron, chiefly as carbonate of protoxide, with 28 per cent of earthy substances. When the ore is roasted in the mine-kiln the water is driven off, a part of the sulphur is expelled, and, if properly performed, the carbonic anhydride is also expelled, whilst peroxide of iron is produced. Two modifications of this process have been suggested: one is to get rid not only of carbonic anhydride and water, but also of the oxygen of the ore before its introduction into the blast-furnace; and the other is to charge the ironstone just as it comes from the mine. Mr. Bell admitted that the application of any method which would remove from the blast-furnace every trace of carbonic anhydride would cause the process to be less dependent upon the nature of the sources of heat than it is at present; but he considered that the additional cost of deoxidising the ironstone would be too great to render its adoption profitable. The author also noticed some improvements recently attempted in the manufacture of coke, the object being to increase the percentage of fixed carbon obtained from a given quantity of coal. The experiments had not, however, given successful results.

At the same meeting Mr. W. Crossley read a paper "On the Manufacture of Iron in the Hæmatite District." In describing the blast-furnaces used in Furness, he stated that the bell-and-hopper arrangement used for taking off waste gases was not extensively employed in that district, as it was commonly supposed to act prejudicially on the quality of the iron, and to throw a heavier back pressure on the furnace—objections which the author does not hold to be valid. Two kinds of hæmatite are used, one being hard and dry, and employed for producing pig-iron; whilst the other is soft and damp, and is used for fettling. As the ore is rich in silica and destitute of alumina, it is thought desirable to mix with it some of the Irish aluminous ores in order to produce a good flowing slag. In smelting the ore, it must not be charged in too large pieces, and must be properly distributed through the furnace, whilst from time to time the contents of the furnace require to be gauged.

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* The Manufacture of Russian Sheet-Iron. By John Percy, M.D., F.R.S.; London: Murray. 1871.

We learn from the "Mining Journal," that some little stir has been excited by the recent discovery in Low Furness of an enormous deposit of hæmatite, estimated by competent authorities at not less than one million tons.

At the Round Oak Ironworks, belonging to the Earl of Dudley, Howatson's puddling and heating furnaces have lately been tested with excellent results. The peculiarity of these furnaces consists in supplying hot instead of cold air for the combustion of the fuel, whereby a great saving of coal and iron is said to be effected, whilst the working of the furnace is improved and quickened. In the heating-furnaces the cold air is caused to enter a flue or heating chamber surrounding the base of the stack. Having become heated by contact with the sides of this flue, the air traverses a series of horizontal flues, parallel to each other under the bed of the furnace, whence it passes to the ash-hole beneath the fire-grate, and thence through the fire-bars to the fuel. To ensure perfect combustion of the gases from the fuel and to prevent smoke, a vertical flue is made in the side walls of the fire-grate, and is connected with a horizontal flue furnished with perforations above the bars, which admit and distribute the heated air over the fire.

In the puddling-furnace the cold air is first admitted under the bed of the furnace, which it helps to cool, and then proceeds to a flue surrounding the base of the slack, whence it passes along the sides of the furnace to the end, and finally descends to the ash-pit.

At the time we write, the Iron and Steel Institute is holding a meeting at Dudley, under the presidency of Mr. H. Bessemer. Some interesting papers on metallurgical subjects are before the Institute, and will be duly noticed in this journal.

MINERALOGY.

So rarely has the diamond been found in a veritable matrix that considerable interest attaches to any fresh instance of its occurrence *in sitû*. According to Professor P. von Jeremejew,* of the Mining Institute of St. Petersburg, microscopic crystals of diamond occur as enclosures in the mineral called xanthophyllite. Specimens of this mineral from the Schischimskian mountains, in the Slatoust mining district in the Urals, have been found to enclose crystals which present the form of the hexakistetrahedron, combined with a slightly developed tetrahedron—the faces of the first form being distinctly curved, whilst those of the latter are perfectly plane. The greater number of these crystals are colourless and transparent, but some few are tinted brown. Those enclosures are most abundant in the greenish plates of xanthophyllite, which occur in the neighbourhood of nodular aggregates of talcose schist and serpentine; moreover, these two rocks also enclose similar microscopic crystals.

From the recent researches of Professor Zepharovich, it appears that two distinct minerals have hitherto been confounded under the common name of *Freieslebenite*.* It is well known that much difference of opinion has prevailed respecting the crystalline system to which this species should be referred—some authorities placing it in the rhombic, others in the monoclinic, and others again in the triclinic system. Zepharovich now shows that the native compound, $Ag_4Pb_3Sb_4S_{11}$, is dimorphic, and forms two distinct species, which differ not only in crystalline form, but also in specific gravity. One of these minerals assumes monoclinic shapes, and has a density of 6·35; whilst the other crystallises in the rhombic system, and has a density of only 5·9. For the former species the name Freieslebenite is to be retained, whilst for the latter Zepharovich proposes the name *Diaphorite*, from διαφορα, "a difference." The mineral which has formed so large a part of the ore raised at the silver-mine of Hiendelaencina, in Spain, belongs to the species Freieslebenite, which is found also at Freiberg, in Saxony. Diaphorite occurs at Przibram, in Bohemia, and at Bräunsdorf, near Freiberg.

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* Jahrbuch für Mineralogie, &c., 1871, Heft 3, p. 275.
+ *Ibid.*, p. 277.

Much light appears to be thrown upon the probable origin of certain native metals by some researches by Mr. W. Skey, of the Geological Survey of New Zealand.* These experiments show the powerful reducing action which ordinary metallic sulphides exert upon most salts of gold, silver, and platinum. For example, a single grain of iron pyrites was found competent to reduce 8⅛ grains of gold. The author concludes that although organic matter, as commonly supposed, has probably played some part in effecting the reduction of certain native metals, yet most gold and silver deposits—especially those deposits which occur in the deeper-seated rocks—owe their origin entirely to the deoxidising effects of metallic sulphides upon solutions of salts of the precious metals.

The mineralogist not less than the physicist is interested in the elaborate researches on the pyro-electric properties of the topaz which have been undertaken by Herr W. Hankel. No fewer than sixty-four crystals from Saxony, Siberia, Brazils, and Asia Minor have been subjected to examination. The frequent association of pyro-electric properties with hemimorphic forms of crystal led Haüy to suppose that the topaz, like tourmaline, must be hemimorphic. Such is not, however, the case, and Hankel's studies point to the general conclusion that pyro-electricity is by no means necessarily correlated with hemimorphism, but appears to be a general property of all crystals. At the same time it is found that the distribution of electricity in a crystal varies according as it is hemimorphic or not. In hemimorphic crystals the opposite extremities of the principal axis are crystallographically dissimilar, and hence assume opposite polar conditions; whilst in crystals which are not hemimorphic the two ends are physically similar, and consequently exhibit the same polar condition. In the latter case the distribution of electricity depends in great measure on the external form of the crystal, and may be modified by altering its shape; but in hemimorphic crystals the distribution appears to depend essentially on the asymmetry of the molecules, and therefore suffers no alteration by any change in the external form of the crystal.

Another physico-mineralogical paper claims a brief notice here. Professor Dove has examined the behaviour of different kinds of native silica when freely suspended in the magnetic field. Sections were cut from pure and colourless rock-crystal, from smoky quartz, and from agates composed of alternating layers of chalcedony, jasper, amethyst, and other varieties of quartz. The sections were successively suspended between the poles of a powerful electro-magnet, and their deportment completely established the *diamagnetic* character of silica in all its varieties.

Professor How, of Windsor, Nova Scotia, continues his researches on the mineralogy of this part of the Canadian Dominion, and has published the description of another new borate found in the gypsum quarries of Winkworth, in Hants County. *Winkworthite*, as he terms the new species, is found in nodular masses, more or less crystalline on the exterior, and breaking with a flat fracture which presents irregular glistening facets. Scrapings, viewed under the microscope, appear as transparent oblique-angled plates. Two specimens yielded slightly different results; the one containing $11CaO.SiO_2.9SO_3.3BO_3.20HO$, and the other $11CaO.SiO_2.8SO_3.4BO_3.20HO$. Winkworthite is consequently intermediate in composition between selenite and Howlite, or silico-borocalcite, and may have originated from the reaction of the elements of these two bodies, either during deposition or subsequently.

Under the name of *Cæruleolactine*, Petersen describes a new species of hydric-aluminium phosphate found in the mine of Rindsberg, near Katzenellnbogen, in Nassau. The mineral occurs in a deposit of brown iron ore, which, however, is not worked in the neighbourhood of the cæruleolactine in consequence of the presence of phosphorus. The new species presents a bluish milk-white colour, whence the name, and appears to stand between Wavellite and Kalaite—approaching to the former in its chemical, and to the latter in its physical characters. It is instructive to compare the formulæ of these three species:—

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* **Chemical News, vol. xxiii., p. 232.**

Kalaite	$2Al_2O_3.P_2O_5+5H_2O$.
Cæruleolactine ..	$3Al_2O_3.2P_2O_5+10H_2O$.
Wavellite	$3Al_2O_3.2P_2O_5+12H_2O$.

Dr. Petersen has also analysed the mineral described by Breithaupt as *Variscite*, and finds it to contain $Al_2O_3.P_2O_5+4H_2O$. It appears to be closely related to Damour's *Callais*—a material which is found, worked into ornamental objects, in the old Celtic graves of Brittany.

The New Zealand *nephrite* or *jade*—a substance so largely used by the Maories for purposes of ornament—has frequently been subjected to scientific examination, but often with discordant results. Dr. Kenngott has availed himself of some specimens recently obtained from a large block sent to Germany, and his microscopic and chemical examination leads him to conclude that the different forms of New Zealand jade are micro-crystalline or imperfectly-slaty varieties of grammatite.

A new fluoride from Arksutfiord, in Greenland—the celebrated locality for cryolite—has been described by Professor G. J. Brush under the name of *Ralstonite*. It is essentially a hydric aluminium fluoride, crystallising in colourless or white octahedra, and somewhat resembling in its general characters the rare Cornish mineral—fluellite.

Monzonite is the name which Von Kobell has applied to a new mineral from Mount Monzoni, in the Fassa Valley, Tyrol. It occurs in compact masses, of a pale green colour, somewhat resembling a green hornstone, but is readily fusible. Analysis shows it to be a new silicate of alumina, protoxide of iron, lime, magnesia, soda, &c., referable to the formula $2(3RO.2SiO_3)+2Al_2O_3.3SiO_3$.

At the celebrated salt-mines near Stassfurt, in Prussian Saxony, crystals of *boracite* have recently been found in tetrahedral forms.

An analysis of *allophane* from the oolites of Northampton has been published by Mr. Herman in the "Quarterly Journal of the Geological Society." Mr. D. Forbes in examining this mineral has detected phosphoric acid.

Professor Rammelsberg publishes an analysis of the meteoric stone of Chantonny; together with short memoirs on the sulphide of iron occurring in meteorites, and on the composition of Lievrite.

Papers by Professor Maskelyne on Dufrenite and a new mineral from Cornwall, and on the localities of Dioptase, were read before the Chemical Section of the British Association. In compliment to Professor Andrews, the President of the Section, the name of *Andrewsite* has been bestowed upon the new Cornish species. It occurs in globular forms, or in discs with a radiate structure, and presents a bluish-green colour. Its sp. gr. is 3·475, and its composition may be thus formulated:—$3\{Fe_2P_2O_8+Fe_2H_6O_6\}+CuP_2O_8$. As the only locality hitherto recorded for *Dioptase* has been the copper mine of Altyn-Tubeh, in the Kirghese Steppes, it is interesting to learn that Professor Maskelyne has found this rare species in old specimens from several localities in Chili.*

ENGINEERING—CIVIL AND MECHANICAL.

Guns and Armour.—Since the first introduction of heavy iron plating for the protection of our ships and forts, a continuous rivalry has been going on between guns and armour-plating. The experiments undertaken when a new gun was introduced, or a new kind of armour-plating invented, resulting at one time in favour of the former, and at another time in favour of the latter. At present guns have achieved the last victory over a new target designed by the War Office authorities. It is uncertain, however, at present, whether this success is to be attributed solely to the superiority of the guns and projectiles, or to the inferiority of the target; and referring to the great success achieved by the Milwall shield, in July, 1868—in which the armour-plates were backed with Mr. Hughes's 7-inch hollow stringers, placed horizontally, whilst the rear skin-plates were strengthened by hollow stringers placed vertically—it is just possible that the present victory of the guns may be due more to the inferiority of the target than to their own merits. This question may, however, shortly receive a practical solution, as we understand that Mr. Hughes intends to send another and a still better shield to Shoeburyness for trial. The experiments with the new War Office target took place at Shoeburyness last July. The target measured 48 feet long by 9 feet high, and although built up in one, it really represents two systems of targets. In one the armour-plating is 8 inches thick, with a backing of teak 18 inches thick, and a ¾-inch iron skin with iron ribs in the rear. In the other the front plate is 8 inches thick, backed by 5½ inches of teak, behind which is a 5-inch armour-plate backed with 6 inches of teak and a 1½-inch iron skin. The guns brought to bear upon this compound target were the 9-inch Woolwich muzzle-loading rifled 250-pounder, and the 11-inch Woolwich muzzle-loader rifled 500-pounder gun. The 9-inch gun was directed against the 8-inch armoured portion, and the 11-inch was laid against the target carrying the 13 inches of divided armour, the ranges being in both cases 200 yards. Both guns penetrated the targets, the 11-inch weapon doing exceptionally good work in sending its projectiles through the two armour-plates and backing of the second target. Both plates were cleanly penetrated, the puncture disclosing the fact that the metal was of splendid quality. The second part of the programme for the day consisted of comparative trials with the nominal 4-pounder breech-loading Prussian rifled field-gun, fired with 9 lb. cylindrical projectiles, against the English muzzle-loading 9-pounder and 16-pounder field-guns. The general results of this practice were that the English 16-pounder fired 25 rounds in 13 minutes 30 seconds, making 14 hits upon the target; the English 9-pounder fired the same number of rounds in 8 minutes 37 seconds, scoring 13 hits, whilst the Prussian field-gun fired a like number of rounds in 10 minutes 15 seconds, making 13 hits. Competitive practice was then carried out with the three guns against targets representing troops with good results.

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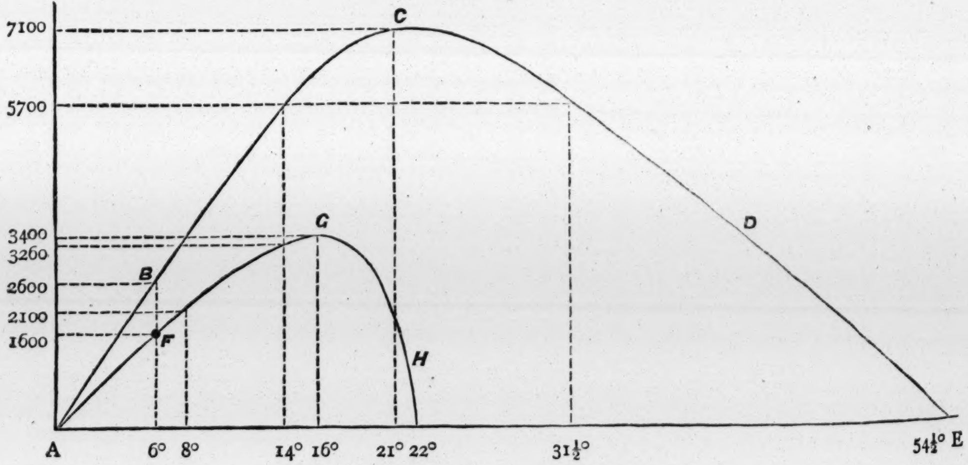
* Chemical News, vol. xxiv., p. 99.

Stability of Ships.—This subject has engaged a considerable amount of consideration since the unfortunate loss of the *Captain*. Amongst other contributions towards a full investigation of this branch of scientific research, we notice a "Lecture on Sea Waves," by W. J. Macquorn Rankine, C.E., LL.D., F.R.S., delivered before the Royal Institution, which we referred to in the July number of this Journal, and a paper on "The Stability of Ships," by W. H. White and W. John, Fellows and late Students of the Royal School of Naval Architecture, and Members of the Institution of Naval Architects, recently read before that Institution. Mr. M. W. Ruthven, C.E., in a letter recently addressed to some weekly contemporaries, remarks that the *Captain* was evidently lost for want of stability, while the official curve of stability was

FIG. 17.



A B C D, official curve of stability.
F, point fixed by inclining experiment.
F G H, probable continuation of curve.

such that, if the ship had really possessed it, no pressure of wind upon any extent of sail she could possibly have carried could have forced her over. Upon further reflection, Mr. Ruthven found the official curve of stability to be grossly incorrect, and not even agreeing with the inclining experiment which had been made at Portsmouth in order to ascertain the position of the centre of gravity. The inclining experiment, as stated in evidence, proved that 80 tons placed on the deck at 20 feet from the centre line, heeled the ship 6 degrees, and this, of course, was the measure of her stability at 6 degrees. This 80 tons multiplied by the 20 feet give 1600 foot-tons as her stability at 6 degrees; while the curve produced in court showed a stability, at 6 degrees, of no less than 2600 foot-tons. The official curve gave 7100 foot-tons as the maximum stability at 21½ degrees, with a stability of 5700 foot-tons at 14 degrees, which was retained up to an angle of 31 degrees; while, in point of fact, the highest real stability of the *Captain* was only about 3400 foot-tons at 16 degrees, and 3260 at 14 degrees, which she carried only up to 18 degrees. The accompanying diagram shows the difference between the assumed and the real curve of stability of the *Captain*, as explained by Mr. Ruthven.

Stone Caissons.—A new application of Ransome's patent concrete stone has recently been introduced by a Mr. Butler, which seems likely to come into extensive use, and to effect a great revolution in the construction of hydraulic works. The object of this invention is to employ cast-stone caissons in the construction of foundations for piers, bridges, river-walls, and all kinds of hydraulic works. The rapidity and ease with which blocks of any form and size can be produced on the spot where they are to be employed, are no small considerations in favour of this new application of Ransome's stone, whilst the materials which form its base are generally found in abundance where hydraulic works are carried on. This application of Mr. Ransome's process has been suggested for two reasons—the first, to provide a cheap and thoroughly efficient substitute for stone for hydraulic works; and, secondly, to render unnecessary the construction of false works, coffer-dams, &c., and to avoid the employment of iron-cylinders and caissons, now of necessity so extensively used. Experimental tests have shown the Ransome stone to be second only to granite in its powers to resist a crushing force. The strength of granite to resist crushing varies from 8000 to 12,000 lbs. per square inch; the Ransome stone, 8960 lbs.; Bramley Fall, 5120 lbs.; and Portland stone, 2630 lbs. per square inch. In practice, the materials forming the stone will be moulded *in situ* into blocks, either solid or cellular, of the required shape and dimensions. The cellular blocks form, however, the special peculiarity of this system. For bridge-piers and abutments the blocks may be rectangular or circular, for dock- and river-walls they may be square or hexagonal—in fact, any required shape may be given to them. For convenience in sinking the blocks (an operation which is precisely similar to that employed in sinking iron cylinders) the lower edge of the bottom length would be chamfered, and, when necessary, shod with iron. The horizontal joints would be made preferably with alternate projections and depressions in the sides of the blocks, and the vertical joints are made good with timbers halved into each block.

Selenitic Mortar.—A most valuable addition to the building arts has recently been invented by Colonel Scott, R.E., of South Kensington, and called by him Selenitic Mortar. The process of production consists in mixing with the water used in the preparation of the mortar, a small quantity of sulphate of lime in the form of either plaster-of-paris or gypsum, or by adding green vitriol. The mixture is prepared in the pan of an ordinary mortar-mill, in which the water and the sulphate are first introduced, and subsequently the lime. After the lime has been ground for three or four minutes, the sand, burnt clay, or other ingredients are added, and the whole are ground for ten minutes more. By this invention, ordinary lime can be at once converted into a species of cement-mortar which sets rapidly and well, and can be used for concrete, bricklayers'-work, or stuff for plastering, at a cheaper rate than that made from lime in the ordinary way. The use of sulphuric acid has been found to give the best results, although sufficient acid is contained in plaster-of-paris to prevent the lime from slaking, which in effect is the secret of the whole process. The lime, by this means, is enabled to take twice as much sand as when slaked. Experiments made against Portland cement, showed that while a joint made with the latter, after standing fourteen days, separated with a weight of 56 lbs., the cement in most cases coming clean away from the tiles which were joined together by it; the selenitic mortar, composed of 1 part lime to 5 of sand, under precisely similar conditions, required a weight of 158 lbs. to overcome adhesion, and then the fracture took place completely through the cement, half remaining on each side.

Narrow Gauge Railways.—The great revolution in the present day as regards the means of locomotion, consists in the adoption of a narrower gauge, and cheaper class of railway than that employed on the first introduction of railways. The rare occurrence of first-class railways proving remunerative has rendered it more and more difficult to raise the necessary funds for their construction, at the same time that the demands for an extension of railway communications have rapidly increased, and so, notwithstanding the opposition to any such change by many leading members of the profession, narrow-gauge lines having steeper gradients and sharper curves than were formerly considered admissible may now be considered as an accepted necessity. In India, a gauge of 3 feet 3⅜ inches has been adopted after very deliberate consideration by a committee appointed to report on the subject, and this will speedily become the standard gauge of the country. Australia, Tasmania, and New Zealand are all following in the same course. In Russia, the 3 feet 6 inch gauge has been definitively accepted, and an extensive system of lines on that gauge is now under construction. In Egypt the same width is to be adopted; whilst in the United States more than 2000 miles of narrow-gauge lines are in actual progress, or about to be commenced. California is organising railways on a reduced gauge in all directions, Canada is following in the same direction, and even for Prince Edward's Island contractors have been invited to tender for the construction of a 3 ft. 6 in. line from Casumpec to Georgetown, a distance of 120 miles. Light railways, and narrow gauge railways, are too often confounded together as being one and the same thing, whereas they are in reality totally distinct. The former subject has been recently brought before the Civil and Mechanical Engineer's Society, in a paper read by Mr. William Lawford, M. Inst. C.E. Light railways may often be of the same gauge as our first-class lines, only of a different construction. As feeders to main lines they will doubtless prove valuable adjuncts to the existing system of the country, especially in those districts where the natural features of the land would otherwise require expensive works. One important feature in connection with light railways must not be lost sight of, viz., that it would be impossible to carry on them a large and rapid passenger traffic, such as is now the case with the main lines of the country. A large and rapid traffic means heavy engines, heavy rails, &c.; but for an omnibus or light goods traffic, a light railway, with light permanent way materials, light engines, &c., might be made with great advantage, not only to the travelling community, but also to shareholders. The author then proceeded to give a description of a short line of "light railway" which had lately been constructed for the Duke of Buckingham and Chandos, by means of which the Duke's estates at Wotton are brought into connection with the Aylesbury and Buckingham Railway at Quainton. This line (4 ft. 8½ in. gauge) is six miles in length, with a branch of one mile and a half. It is throughout eminently a line of light works, and, with few exceptions, a surface line, the highest embankment being 12 feet, and the deepest cutting 10 feet. There are no road-bridges, the turnpike and other roads being crossed on a level. The rails are bridge-rails, weighing 30 lbs. to the yard, and are secured into longitudinal creosoted timbers, 6 feet 6 inches long, by means of fang-bolts; there are transoms 4 inches by 4 inches at every 12 feet, kept in their places by a wrought-iron tie-rod, ¾ inch in diameter. The ballast is 10 feet wide, and is 6 inches to 9 inches thick under the bottom of the timbers. The line is only partially fenced, the existing hedges being utilised for that purpose. The estimate for the works is only £1400 per mile, exclusive of the cost of land. A paper on the subject of "Railway Gauges" was read by Mr. R. F. Fairlie, before Section G of the late

British Association. After tracing the history of the Festiniog Railway, and the labours of the Commission which came over from Russia last year to investigate the subject of communications in this country, Mr. Fairlie proceeded to consider the false economy of the present system of railway traffic management. "I think," he says, "no more striking illustration of the error of our present system can be conceived than is afforded by the daily practice of a magnificent company like the London and North Western Railway, who, at the present moment, be it remembered, have commenced to double the width of their road through press of business, yet who are sending out daily, and daily receiving, at Euston Square, some 4400 passengers in carriages which contain sitting accommodation for 13,500, and who carry their enormous freight in increments, averaging less than one ton, in waggons having six times that capacity." Taking into consideration the real requirements of traffic, Mr. Fairlie comes to the conclusion that "the conditions under which a railway should be laid out to meet these requirements are clearly not those which rule the present system; ample experience proves the contrary, showing that no line, however full of business, can be worked to its full capacity. We are led, then, unmistakably to a narrow gauge, to the adoption of passenger-carriages which shall be filled, of waggons which shall be almost fully loaded, and of weight which shall bear a reasonable proportion to their capacity, and we are led to the adoption of very long trains and powerful engines."

Traction Engines.—The adoption of traction engines for farm purposes, and of road steamers for short traffic purposes on our high roads, is gaining considerably in favour, and each year adds to the purposes for which these engines prove their capacity. At the recent show of the Royal Agricultural Society, at Wolverhampton, competitive trials took place between engines constructed by different makers, and the various forms of wheel-tyres—elastic and non-elastic—were amongst the most interesting features connected with them. A paper on the subject of "Road Steamers" was read by Mr. R. W. Thomson, before Section G of the British Association, in which the trials at Wolverhampton were referred to, besides other evidences of the uses to which these engines are now applied with advantage. The question of traction engines or steam locomotives to work on common roads is of much more ancient date than railways; the difficulties that had to be overcome, however, proved at that date too great, and they were for the time set on one side in favour of railways. These latter, however, failed to supply certain local requirements, and engineers again set to work to produce a suitable road engine. At one time it was found that the rough road broke the machinery, and the engine was then made so heavy that it destroyed the road; and when it was found that the surface of the ground would offer no hold to the wheels, the wheels in revenge, as it were, were provided with claws which gripped the ground, but tore it woefully. Mr. Boydell then followed with his endless rails, which, though a scientific solution of the difficulty, failed in practice. Mr. Thomson then adopted elastic tyres, and by surrounding the wheels with solid india-rubber, 4 inches or 5 inches in depth, succeeded in overcoming in some degree the evils attending inequalities of road-surface, and making it a matter almost of indifference whether the road be hard or soft; the only kinds of soil on which the india-rubber tyres cannot work being ground so soft as to flow away from under the wheel, or wet clay, which has a tendency to ball upon the wheels, and so impede the action of the india-rubber. When used for farm purposes the width of the tyre is an important consideration; it has been found that an engine weighing nearly 7 tons, mounted on tyres 9 inches wide, could not move itself over a damp, heavy clay field; whilst an engine weighing 6½ tons, but mounted on tyres 12 inches wide, was not only able to travel itself with ease over this field, but was also able to haul the engine with narrow tyres and a 3-furrow plough, which was attached to the narrow-tyred engine in such a way that it could not be easily removed.

TECHNOLOGY.

The difficulty of uniting iron to brass is caused by the unequal rate of expansion in the two metals, which destroys the unity when the temperature is changed. A new alloy of copper is announced, and the inventor claims that its expansion by heat is so similar to that of iron and steel, that the surfaces may be regarded, when joined, as permanently united, for all practical purposes. The formula is as follows:—Tin, 3 parts; copper, 39½ parts; zinc, 7½ parts.

A new plan for meat preserving has been been introduced by an engineer, whose experience in sugar refineries and other extensive works in hot latitudes has ensured a practical and economical solution of one of the most important problems of the day. Mr. T. F. Henley does away with steeping meat in water, and with boiling and otherwise treating it in the most costly way. He simply squeezes a definite amount of juice out of the fibre, and by mechanical desiccation preserves the latter intact. The pressed meat thus obtained contains 10 per cent of alcoholic extract and salt, and over 50 per cent of fibrin and other albumenoid constituents. It is exceedingly rich, and so is the meat-juice, which Mr. Henley evaporates in vacuum pans. The juice contains about 15 per cent of alcoholic extract, and over 50 per cent of albumen. The ancient method of abstracting water only from the animal matter is relied on as the preservative, and the low temperature at which the evaporation is carried on prevents any loss of flavour or other deterioration. It is perhaps strange that so cheap and simple a process should not have been suggested before. Mr. Henley has worked at it for some time, and perfected it so as to ensure its immediate adoption. The first works, on an extensive scale, are to be opened in the River Plate, on the Estancia Nueva Alemania, where cattle have been reared and fattened for the European markets.

Acetate of alumina has been found very useful for the purpose of rendering woven fabrics waterproof without thereby impeding the passage of perspiration. Professor Balard prepares the acetate of alumina by dissolving 30 grms. of acetate of lead in ½ a litre of water, and also 24 grms. of sulphate of alumina in ½ a litre of water. These solutions, having been mixed, are next filtered, after which the fabric is immersed therein for a quarter of an hour, and, after having been well drained, is dried in the air.

Dr. E. Kopp has published a lengthy memoir containing, in a condensed form, all the information on the subject of distinguishing silk, wool, and vegetable fibres from each other, and also the various methods employed on the large scale for separating animal and vegetable fibres when they occur in mixed fabrics. Among some of the particular reactions we find the following for detecting wool in silk, and *vice versa*, based upon the fact that wool contains sulphur, while silk does not. The tissue to be tested—it should be, however, white, not dyed—is put into a solution of caustic potassa or soda, wherein oxide of lead has been previously dissolved; woollen fibres become black when immersed in this liquor, whereas silk remains unchanged. Another test for the same purpose is ordinary nitric acid, which dissolves silk in the cold, but hardly affects wool.

The following mixtures have been found, after a series of experiments, to be the best for red, green, and blue Bengal lights:—For red, 9 parts of nitrate of strontia, 3 parts of shellac, 1½ parts of chlorate of potassa; for green, 9 parts of nitrate of baryta, 3 parts of shellac, 1½ parts of chlorate of potassa; for blue, 8 parts of ammoniacal sulphate of copper, 6 parts of chlorate of potassa, 1 part of shellac. This latter ingredient need only be coarsely pulverised. The mixtures here alluded to are suitable for use in theatres and rooms, as by the combustion no injurious vapours are given off.

Dr. F. Springmühl has described a series of experiments made with the view to ascertain how far, and under what conditions, sodium may be employed as a substance suited to cause, by contact either with water or other materials, the explosion of vessels wherein these substances are contained. The force exerted is by no means small, as may be inferred from the following:—46 grms. of sodium and 18 grms. of water yield 2 grms. of hydrogen, a bulk of 22471·9 cubic centimetres. The space required for the sodium only amounts to 44·7 c.c.; the water is contained, previous to its coming into contact with the sodium, in a small bulb, which is solidly fixed by means of a neck to the bulb wherein the sodium is contained, which latter may be made of 50 c.c. capacity. Taking 50 c.c. for the capacity of the explosion bulb, the pressure of the gas generated inside will be 450 atmospheres, equal to 6800 lbs. to the square inch.

From a lengthy memoir by Mr. P. Champion on the properties and industrial manufacture of nitroglycerine, we quote the following particulars. Nitroglycerine is absolutely insoluble in water, but soluble, in all proportions, in ether, methylic alcohol, and ordinary alcohol (but, as regards the latter, only at a temperature above 50° C.) Nitroglycerine is somewhat volatile, without decomposition, above 100°, and is not liable to spontaneous decomposition when pure. When exposed for several hours to a temperature of $-15°$, nitroglycerine becomes thick, but not solid; while a prolonged continuation of cold of only $-2°$ freezes this body, converting it into a crystalline mass. Fuming nitric acid dissolves, but also decomposes, this fluid; and the same effect is produced by concentrated sulphuric acid, and also by the mixture of sulphuric and nitric acids employed for the preparation of nitroglycerine. This latter fact explains the deficiency of the theoretical quantity (246) which is obtained in the preparation of nitroglycerine from 100 parts of glycerine. The author's experiments prove that pure nitroglycerine boils, but does not explode violently, at 185° C.; volatilises slowly at 194°, rapidly at 200°; deflagrates violently at 217°; detonates difficultly at 241°, but violently and completely at 257°; at a higher temperature the detonation is less violent, and at 287° is accompanied by flame. At a low red heat, nitroglycerine assumes the spheroidal state, and is volatilised without detonation. While nitroglycerine detonates with great violence by a smart blow it is not affected by electrical shocks.

The substance known as dynamite (consisting essentially of nitroglycerine absorbed by any suitable inert powder) is met with in the trade made up in cartridges weighing, on an average, 71 grms., and packed in boxes weighing with contents, about from 25 to 30 kilos. M. P. Guyot happening to have in his possession a number of cartridges containing dynamite (the body of the cartridge is made of stout grey paper), found that these objects, after some lapse of time, became moist and oily looking, and a cardboard box in which the cartridges were kept was also found impregnated with a liquid, which, on investigation, turned out to be nitroglycerine. A small piece of the paper so impregnated exploded violently when brought into contact with glowing coals, and the like effect was observed when a piece of the paper was laid upon an anvil and smartly struck with a hammer. The author also found that the wood of the boxes in which dynamite cartridges are kept becomes, by slow degrees, impregnated with nitroglycerine, and thereby a most dangerously explosive material, which may give rise to serious accidents in warehouses where dynamite is kept.

In a subsequent publication the same author proposes that instead of paper, parchment should be used for making the cartridges filled with dynamite, so that no nitroglycerine can escape by soaking the paper, parchment being impervious to the liquid just alluded to.

In a memoir on the accidental and spontaneous explosion of explosive substances, and on a preventative of such occurrences, Dr. Zaliwski says that the explosive property of the inflammable substance depends upon the hygrometric condition of the atmosphere—that is to say, that gunpowder and other explosive materials may become, even without elevation of temperature, spontaneously explosive, but that the smallest trace of oxalic acid is, if mixed with the explodents, sufficient to prevent spontaneous explosion due to the cause alluded to, owing to a catalytic effect which precedes the loss of basic water of the acid alluded to. The author further says that this fact can be readily experimentally proved by adding, to a mixture of sulphur and chlorate of potassa, for instance, or to any other explosive mixture, a certain quantity of oxalic acid, after which these materials may be heated even up to their point of fusion without exploding.

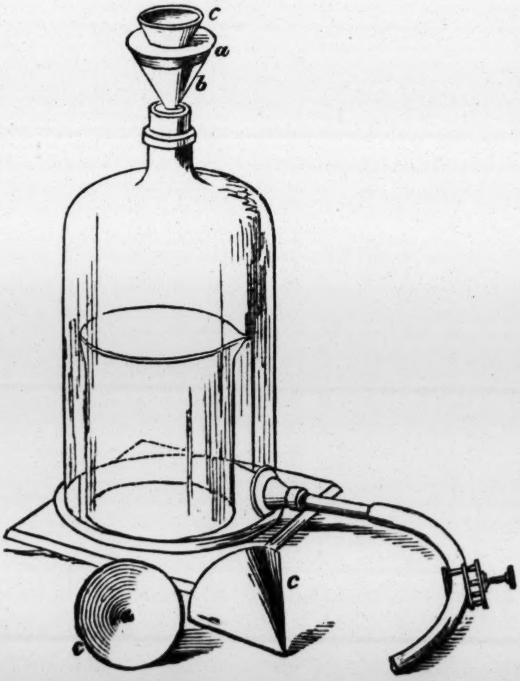
At a comparatively remote period, saltpetre was regularly manufactured in most European countries by a process too long to be further alluded to here, and well known, undoubtedly, to most of our readers. MM. Thiercelin and Willm state that the richness of the saltpetre-yielding materials recently tested by them varies very much. They found that the efflorescence now and then met with, especially on the walls of old stables, contained as much as 67 per cent of saltpetre; but this material is rare. Old wood-ash yielded ½ per cent; the mortar of a wall covered with ivy yielded 2·6 per cent; and the mortar of the same wall at a spot not covered with that plant, 4·6 per cent of saltpetre.

CHEMICAL SCIENCE.

The following test for the detection of small quantities of sulphur present in coal-gas has been proposed by Dr. V. Wartha. Form first before the blowpipe, in the loop of a platinum wire, a bead of pure soda, and next pass this bead over the edge of the gas flame, after which the bead is held in the interior of the flame in order to deoxidise the sulphates and sulphites of soda into sulphuret of sodium; the bead is then transferred to a porcelain basin, crushed, and some nitro-prusside of sodium added, whereby the smallest trace of sulphur will be detected. This reaction is fifty times more sensitive than that upon silver-foil; and the test can be performed in about three minutes, whereas Dr. Vogel's sulphur-copper reaction for this purpose takes four hours.

The idea of using a cone made of very porous eathenware, and as a substitute for the paper filter, has been carried out by Mr. C. E. Munroe in the following manner:—The cones are made of very light, porous earthenware, and have an angle of about 60°. They are used in the following way:—A section of a seamless rubber tube, *a*, is stretched around the mouth of a funnel, *b*, preferably a Bunsen funnel, allowing a portion of the tube to project above the top. This part will immediately arrange itself at right angles to the top of the funnel; into the circle thus formed the cone, *c*, is put. It is then connected with the Bunsen pump. When the cone is moistened and the pressure applied, the rubber band forms an air-tight joint, and the liquid runs through with great rapidity. Before the cones are applied to quantitative work they must be carefully washed, first with concentrated hydrochloric acid, then with distilled water, dried, and weighed. A small porcelain crucible is kept at the balance in which to weigh them. The cones can be used repeatedly, and can replace paper filters in every case. They will, undoubtedly, be found to be of great value in commercial work, for drying crystals and filtering corrosive liquids. As they will stand sudden changes of temperature without breaking, they can be substituted to advantage in many cases for crucibles.

FIG. 18.



When solid nitrate of silver, either in crystals or sticks, is placed upon glowing charcoal, deflagration takes place, the silver being left in the metallic state, while binoxide of nitrogen and carbonic acid are evolved. The nitrate is fused by the heat of the reaction and sinks into the pores of the charcoal, and as each particle of charcoal is replaced by metallic silver, the structure of the original wood is preserved. With proper management, pieces of silver of any desired size can be prepared, showing the exact structure of the wood. Dr. Chandler recommends that a crystal of nitrate be placed on the end of a piece of charcoal, and the blowpipe flame directed upon the coal near the crystal to start the reaction. When deflagration begins, crystal after crystal may be added. The nitrate fuses, passes down through the porous metal already reduced until it reaches the glowing coal, where it is reduced. Lumps of silver weighing an ounce or more, which exhibit most beautifully the rings of the wood, have been prepared in this manner.

The behaviour of arsenic acid with hydrochloric acid, with a view to obtaining an acid free from arsenic, has been examined by J. Mayrhofer. His paper contains the record of the results of some experiments made with the view to ascertain under what conditions and in what state of combination arsenic is carried over by the distillation of hydrochloric acid which is contaminated with arsenic. It appears that the degree of concentration of the last-named acid, and the relative proportion of arsenic acid present, and also the care taken in cautiously distilling, are of influence in obtaining an arsenic-free distillate; but, moreover, the treatment of the hydrochloric acid with chlorine, or, better still, with sulphuretted hydrogen, previous to distillation, to be next carefully conducted, will ensure the distillate being free from arsenic.

In some recent experiments in chemical dynamics, Dr. J. H. Gladstone, F.R.S., and Alfred Tribe, F.C.S. had occasion to study the action of nitrate of silver on copper plates in various positions. They observed that, when the plate was vertical, there was rather more corrosion at the bottom than at the top. This is easily accounted for by the upward current, which flows along the surface of the deposited crystals, and which necessitates a movement of the nitrate of silver solution towards the copper plate, especially impinging on the lower part. It was also found that, when the copper plate was varnished on one side, it produced rather more than half the previous decomposition, and was most corroded at the edges of the varnish. By making patterns with varnish, this edge action became very evident. This was explained by the fact that the long crystals of silver growing out from the copper at the borders can spread their branches into the open space at the side, and so draw their supply from a larger mass of solution than the crystals in the middle can do; and increasd crystallisation of silver means increased solution of copper. This was proved by making the varnish a perpendicular wall instead of a thin layer, when the greater corrosion was not obtained. In a plate completely surrounded with liquid, the greatest growth of crystals is also evidently from the angles. It was likewise observed that, if a vertical plate be immersed, the lower part in nitrate of copper and the upper part in nitrate of silver, there is greater corrosion about the point of junction; this was attributed to the greater conduction of the stronger liquid.

A new precipitating reagent for copper has been proposed by Mr. Hugo Tamm, which promises to be of great service in analysis. Such a reagent, to be perfect, must fulfil certain conditions. (1). When employed for the determination of a solid substance it should be volatile, or, if it is fixed, it should not form, with the precipitate, compounds of indefinite composition. (2). It should form, with the element to be determined, a compound as insoluble as possible. (3). It should not introduce in the liquid separated from the precipitate any substance likely to alter the behaviour of its constituents, with general or respective reagents, and, least of all, it should not introduce any substance difficult of separation from any of the constituents. (4). It should separate the element to be determined, in the shape of a compact precipitate, in neutral or acid liquors, and allow the other elements of the combination to remain in solution. This is, perhaps, the most important principle in analytical chemistry. The reagent which Mr. Tamm proposes for copper fulfils these conditions, and is, consequently, very perfect; in practice it will be found very useful. It is obtained by dissolving in distilled water equal weights of sulphocyanide of ammonium and of bisulphite of ammonia. This mixture keeps well, and can be used several months after its preparation, providing it is not left exposed to the air for any length of time; but, should a slight alteration take place in its composition, this would be of no consequence. When added to a solution containing copper, it immediately precipitates white sub-sulphocyanide of copper as an insoluble powder readily washed, whilst it precipitates scarcely any other metal which may be present.

The difficulty in burning off organic matter has often been experienced by analytical chemists, especially when the substance under incineration yields a readily fusible ash. M. A. Béchamp has proposed the use of nitrate of bismuth in the state of an aqueous solution of known strength to be mixed with the material to be incinerated, provided the water contained in the substance has been previously ascertained by drying at 100°. The nitrate of bismuth solution (the bulk thereof to ignite and calcine readily 100 to 150 grms. of substance should contain from 3 to 4 grms. of oxide of bismuth) having been mixed with a fresh and weighed portion of the substance (yeast is taken in illustration, as it is well known that this is very difficult to burn off completely), the mixture is first dried gently on a water-bath, next heated on a sand-bath hot enough to cause the mass to blacken, after which it burns away as tinder, and, if required, the ignition is completed over the lamp. Should any fear exist that some metallic bismuth has been formed, nitric acid is added to the ash, and the heating repeated, so as to destroy the nitrate of bismuth thus produced. From the solution the bismuth may be removed by sulphuretted hydrogen.

Ch. A. Bœhme relates an occurrence which appears to lead to the conclusion that chloral hydrate may be spontaneously decomposed when kept in sealed bottles. Two of these, each containing 1lb. of the substance alluded to, were obtained from a leading drug-house at New York; one of the bottles was opened at once, and nothing special noted in its contents; the other bottle was placed in a store-room, and on being after some time opened, a dense cloud of fumes was observed to issue from its mouth. These fumes had the characteristic odour of chloral hydrate, but were more stifling, reddened blue litmus-paper, and, on further testing, were found to consist partly of hydrochloric acid. The lumps of the hydrate near the top of the bottle had crumbled to a crystalline powder freely soluble in water, somewhat in chloroform, but insoluble in sulphide of carbon and oil of turpentine. The author's opinion is that the sample alluded to was pure hydrate of chloral at first, but had been decomposed by standing.

A subject of some importance in analysis has been pointed out by Mr. J. Myers, who finds that sulphuretted hydrogen is often contaminated with arseniuretted hydrogen. At the ordinary temperature the two gases alluded to can co-exist without decomposition, which only takes place at the boiling-point of mercury. The author, while experimenting, thought that the arseniuretted hydrogen might be due to the presence of some arsenic in the sulphuret of iron employed, but, on investigation, it turned out that the source of the arsenic was in the commercial sulphuric acid used for the evolution of the sulphuretted hydrogen.

LIGHT.

The fine purple colour of the vapour of iodine has been found by Dr. Andrews, F.R.S., to arise from its transmitting freely the red and blue rays of the spectrum, while it absorbs nearly the whole of the green rays. The transmitted light passes freely through a copper-red or a cobalt-blue glass. But if the iodine vapour be sufficiently dense, the whole of the red rays are absorbed, and the transmitted rays are of a pure blue colour. They are now freely transmitted as before by the cobalt glass, but will not pass through the red glass. A solution of iodine in sulphide of carbon exhibits a similar dichroism, and according to its density appears either purple or blue when white light is transmitted through it. The alcoholic solution, on the contrary, is of a red colour, and does not exhibit any dichroism.

The spectrum of lightning has been examined by Mr. J. Gibbons during a severe thunderstorm, but having only a pocket spectroscope, he was unable to determine the position of the bright bands with any degree of accuracy. Two narrow blue-green bands most frequently made their appearance; they were somewhat narrower than those of the chloride of copper spectrum, and separated by a wider interval. At times when the flashes were particularly vivid, bands of various degrees of luminous intensity and of breadth were distributed over the whole spectrum; and, during the occurrence of a flash *en zigzag*, a group of narrow ones—two being very brilliant—appeared in the more refrangible part of the red, besides those bands in the blue and green, which were always most conspicuous. The complexity of these spectra gave the author the impression that this department of meteorological chemistry would prove a most promising field of investigation.

Microscopy.—The cells of pure tin now extensively employed in mounting microscopical objects have been the cause of some mischief in the case of tissues injected with chromate of lead and mounted in fluid (probably slightly acid). The lead in the coloured injection has been reduced, and can be seen in numerous crystalline patches, while the yellow tint has almost totally disappeared; the whole combination of cell, fluid, and easily reducible metallic salt, probably formed a miniature battery, and hence the spoiling of the preparation. The use of these cells should always be avoided in mounting tissues injected with vermillion, chromate or carbonate of lead, Prussian blue, or other metallic pigments, glass cells being preferable and incapable of doing injury. However, for the majority of mountings in fluid and all dry preparations the metal cells may be used with perfect safety, no preservative fluid at present used by microscopists acting upon them. The above-mentioned case must be regarded as purely exceptional, and by no means as prohibiting the employment of these economical and useful cells.

A fortunate accident has enabled Mr. Wenham to establish the reality of the appearances known as the ! markings on the Podura scale. In a slide of *Lepidocyrtus curvicollis* (the "test Podura") prepared by Mr. S. J. McIntire, a fracture caused by the slipping of the cover has left some of the ends of the much disputed markings projecting; owing to the toughness of Podura scales such fragments are difficult to obtain, and opportunities for such observations seldom occur. The Podura difficulty has been solved in precisely the same manner as that of *nodular* or *beaded* structure of the *Diatomaceæ*, in which the examination of fragments revealed the truth; the varied appearances presented being caused by alterations in the illumination and focus and adjustment of the object-glass. The late R. Beck, Mr. Hennah, Dr. Pigott, and others have shown how easily false appearances are to be produced, especially in transparent bodies, such as glass rods, bosses, &c.

Mr. N. E. Green has employed the lime-light for the purpose of illuminating microscopical objects viewed with high powers by reflected light. The objectives used have been $\frac{1}{6}$th and $\frac{1}{12}$th by Ross, and $\frac{1}{12}$th by Gundlach, of Berlin. The long working distance of the latter objective has been found especially serviceable. The lime-light was placed about six inches from the microscope, and concentrated upon the uncovered object by means of a small condensing lens. The surfaces of *Biddulphia*, *Triceratium*, and *Isthmia* exhibited an appearance compared by Mr. Green to that of a nutmeg grater, but in his drawings closely resembling lunar craters. The "*beads*" or "*hemispheres*" of *Pleurosigma Hippocampus* and *P. formosum* were distinctly seen, and also those of *P. angulatum*, but in this instance it required the most careful adjustment of the light to develope their elevations. Mr. Green explains the conflicting appearances exhibited by these *Diatomaceæ* when examined by transmitted light by considering that the hemispheres really surmount cylinders of a perceptible length, as seems to be proved by examining the edge of a broken or abraded specimen, or by focussing downwards, and these cylinders by compression take naturally the hexagonal form, so that according to the part focussed, and the nature of the illumination employed, dots, lines, hexagons, or beads may be seen.

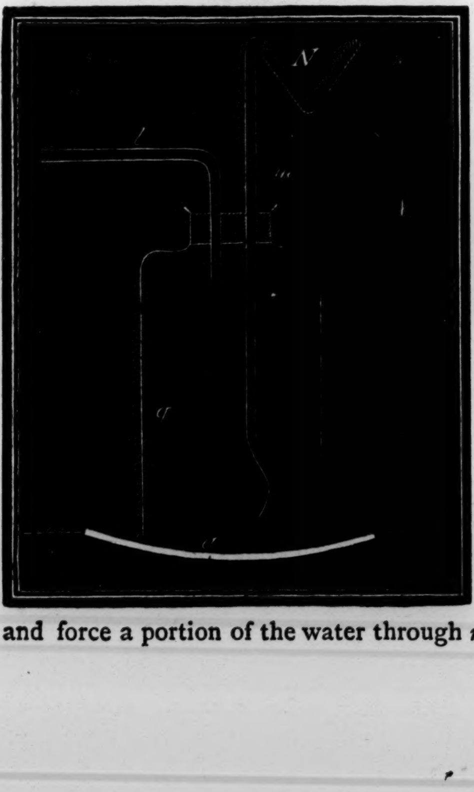
Mr. Sorby makes a valuable communication to the "Monthly Microscopical Journal" for July on the "Spectrum Method of Detecting Blood." After describing the best combination of prisms to be employed in the spectrum-microscope, which are recommended to be of moderate dispersive power, and the cells, and other apparatus and reagents useful in these examinations, he proceeds to demonstrate the characteristics of the blood spectrum, and the changes caused in it by the action of reagents. The changes caused by exposure to damp are next noticed, and valuable information is given respecting the detection of old blood-stains. The action of mordants and dyes in fabrics, and the variations in manipulation caused by their presence, are duly treated upon, and the recognition of blood in very diluted solutions is shown to be possible. The whole paper is of great interest to all workers with the micro-spectroscope and those engaged in medico-legal enquiries.

Mr. Ladd has contrived a polariscope to be used with the microscope in the examination of the rings about the optic axes of crystals, which has the advantage of being less expensive than ordinary appliances for the purpose, and also gives so large a field that both the axes of sugar are easily brought into view. Beneath the stage is placed a pair of lenses somewhat resembling an ordinary Huyghenian eye-piece, but with a nearly hemispherical front glass; this acts as a condenser. A similar combination of lenses is attached to the body of the microscope in the place of the objective; a double convex lens is placed in the bottom of the draw-tube, which is here used for the purpose of focussing; beneath the condenser is placed the ordinary Nicol prism, and a low power eye-piece with a Nicol prism above it as an analyser completes the arrangemet.

HEAT.

In the phenomenon of the spheroidal state, the globule will float when the vapour beneath it is able to support the pressure of the atmosphere plus the weight of the globule. If we remove the former factor, a much smaller vapour tension will be required to produce the phenomenon, as may be proved by the following experiment, described by E. Budde, in which, with the aid of the air-pump, a Liedenfrost globule is supported upon a metal plate whose temperature is below 100° C. A bell-shaped glass vessel, *g*, is firmly cemented to a copper plate, *a*. Through the stopper which closed the upper opening pass two glass tubes, *l* and *m*. The first attaches by caoutchouc tubing to the air-pump. The second reaches within the vessel nearly to the plate *a*, while above it is closed and bent into an N form. The bent portion is filled with water. The plate is now placed upon the water-bath, which soon imparts to it a temperature of from 80° to 100° C. The air-pump is now put in operation; the water in N evolves air-bubble and vapours (gentle heating will facilitate the operation), which mainly accumulate in the upper end of the tube—and force a portion of the water through *m*. The water falls boiling, or very

FIG. 19.



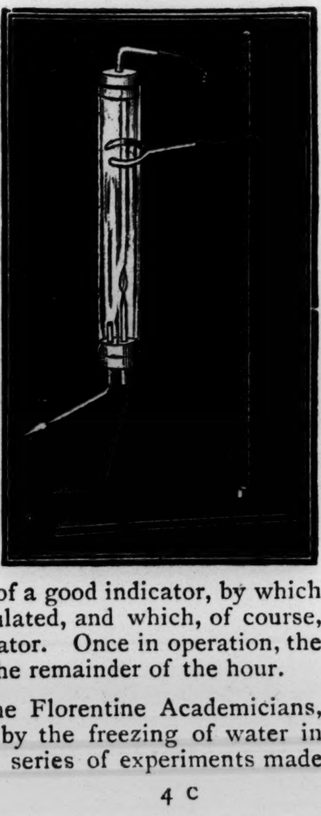
nearly so, upon the plate beneath, the temperature of which is, under the abnormal conditions, considerably above the boiling-point of the water—and all the conditions necessary for the production of the spheroidal state are present. If the rarefaction is carried until the barometer indicates 10 c.m. (about 4 inches) of mercury, and the water-bath is heated to about 90° C., the experiment will succeed without the slightest difficulty, and the spheroids obtained will evince an energetic movement. The experiment is not a mere physical curiosity, but possesses an importance which our educated readers will doubtless have already appreciated, inasmuch as it is decisive in confirming the theory of the spheroidal state. It proves that the force which sustains the globule obeys the laws which govern the tension of vapours.

Dr. Andrews has examined the action of heat on bromine. If a fine tube is filled one-half with liquid bromine and one-half with the vapour of bromine, and after being hermetically sealed is gradually heated till the temperature is above the critical point, the whole of the bromine becomes quite opaque, and the tube has the aspect of being filled with a dark red and opaque resin. A measure of the change of power of transmitting light in this case may be obtained by varying the proportion of liquid and vapour in the tube. Even liquid bromine transmits much less light when heated strongly in an hermetically-sealed tube than its ordinary state.

A number of devices, some of them simple, others complex, have from time to time been described for showing the reciprocal combustion of the elements of water, and experiments of a similar nature. Most, if not all of these, however, as will be found upon testing, either do not entirely remove the danger of an explosion from the operator, or they require the exercise of an unusual amount of care and dexterity to be used with success.

The accompanying arrangement, which is of the most simple character, and which we saw for the first time on the lecture table of Professor Himes, we have since repeatedly used to show the burning of oxygen, air, chlorine, &c., in hydrogen, burning gas, or hydrocarbon vapours. The experiment can be performed with such ease that it is worthy of notice.

Fig. 20.



The arrangement consists of a cylinder of glass, about a foot or a foot and a half in length (the kind used commonly as chimneys for the argand-burner can be had of proper length). This is furnished above and below with a cork; the one at the upper end has one, that below has two, glass tubes of the form shown in figure. The whole affair is supported from the retort stand. The hydrogen (in the H and O experiment) is admitted through the upper tube; when it has completely displaced the air it is ignited below—the cork having been removed—and the supply is regulated until only a weak hydrogen flame remains. The oxygen supplied through the straight tube in the lower cork is now turned on slightly, and the cork fitted into its place. The flame of hydrogen at the opening is extinguished, but the oxygen, in passing up through it, is ignited, and burns now in the centre of the cylinder. The surplus of the hydrogen escapes now from the second tube below, and can be there ignited. This last flame serves the purpose of a good indicator, by which the supply of gases in the cylinder can be regulated, and which, of course, leaves the size of the flame at the will of the operator. Once in operation, the experiment may be left to take care of itself for the remainder of the hour.

After briefly referring to the experiments of the Florentine Academicians, and of Huyghens (1667), on the force exerted by the freezing of water in closed metallic vessels, M. Boussingault relates a series of experiments made by him last winter in order to ascertain whether water, when put into a strong vessel (a steel cylinder of great strength, and so arranged that the dilatation, or expansion, of the water when cooled below + 4·1°, could be prevented), would or would not remain liquid, even when exposed to a cold very considerably below its point of congelation, in consequence of the expansion due to the cooling down from below + 4·1° being prevented by the strength of the vessel containing the water and stopper (steel plug) fitted thereto. The result of this investigation was found to be that water remains liquid under the conditions alluded to, even at a temperature of − 18°, but freezes instantaneously as soon as the impediment caused by the resistance of the plug which hermetically closes the steel vessel, was removed, and free play was given to the expansion of the liquid. It should be noted that the sides and bottom of the steel vessel alluded to were of such great strength as to be practically unyielding.

ELECTRICITY.

E. Bourgoin describes a series of experiments, from which the following main results may be deduced:—When the galvanic current passes through acidulated water, it will be seen that, though the total effective work done by each pole is the same, the quantity of acid found in each compartment at the end of the experiment will be found to vary considerably. The three following cases may be distinguished:—(1) The acid is accumulated regularly at the positive pole; this occurs with sulphuric, nitric, phosphoric, benzoic, succinic, camphoric, &c., acids. (2) There is no loss at the positive pole; the loss is only experienced at the negative pole, since half of the electrolysed acid is regenerated in the other compartment. (3) The two compartments become simultaneously poorer; this occurs with lactic, tartaric, citric, and in general, all the very readily oxidisable acids.

M. Gramme, of Paris, has recently completed a magneto-electric machine from which he obtains a continuous current. The principle is that of passing a permanent magnet through a coil of wire constantly in one direction; or, what is virtually the same thing, keeping the magnet fixed, and causing the coil to move. To effect this M. Gramme employs an annular coil of 200 metres of copper wire of 2 m.m. in diameter, wrapped in silk. By a multiplying wheel and pinion this coil is caused to revolve between two concave armatures attached to a series of permanent magnets. One of these armatures developes in the circuit an induced current in one direction, whilst the other armature induces a current in the opposite direction but of the same intensity. To collect these currents two rubbing-pieces are placed in metallic contact with the coil at the neutral points of the two armatures: one of these contact pieces receives the positive electricity, the other the negative, becoming respectively the positive and negative poles of the electro-motor. By this means and by a rather complicated method of winding the wire on the coils, M. Gramme obtains a perfectly continuous current, capable of decomposing water in a voltameter, and of producing all the results obtained with the battery. Instead of permanent magnets, the inventor in practice employs electro-magnets. In the latter case there is always sufficient residual magnetism to induce a current in the annular coil when motion is imparted, and as in Siemens's and in Wheatstone's instruments this weak current speedily induces a stronger, a maximum of 700 or 800 revolutions per minute can be attained. With a half-horse power, nearly 20 inches of platinum wire one millimetre diameter can be heated to incandescence. The machine will be very useful where a constant current of high intensity is required.

Electricity certainly seems to be entering into nearly all sciences. Sir Charles Wheatstone has at length called in its aid to Hygrometry. A thermopile of five couples has the faces exposed in an ebonite dish in which water or spirit is placed. The reverse faces of the pile are exposed to the air, insulated in solid paraffin; two connecting wires are carried to a delicate galvanometer of a few turns. The variations in deflection of the needle of the galvanometer thus become a relative measure of the evaporation of the liquid contained in the ebonite cup, and by this means very great accuracy can be attained, as the degree of deflection may be read off by a microscope. As yet, we believe, no definite observations have been made with the instrument, which is intended for Kew Observatory.

MM. de la Rive and E Sarasin have communicated to the Société de Physique et d'Histoire Naturelle de Genève a most interesting paper, embodying the results of their experiments on the action of magnetism upon electrified gases. The experiments are detailed at great length, and have already appeared in English in the "Philosophical Magazine." The following are the deductions:—

"1. That the action of magnetism when it is exerted only on a portion of an electrical discharge transmitted through a rarefied gas, determines in that portion an increase of density.

"2. That the same action, when it is exerted upon an electrical discharge placed *equatorially* between the poles of an electro-magnet, produces in the rarefied gas through which it is propagated an *increased* resistance, which is as much greater as the gas itself is a good conductor.

"3. That this action, on the contrary, determines a *diminution* in resistance when the discharge is directed *axially* between the two magnetic poles, this diminution increasing with the conductivity of the gas.

"4. That when the action of the magnetism consists in impressing a continuous movement of rotation on the electrical discharge, it has no influence on the resistance to conduction, if the rotation is effected in a plane perpendicular to the axis of the magnetised soft iron core which determines the rotation; while it considerably diminishes it if the rotation takes place so that the electrical discharge describes a cylinder around the axis of the rod.

"5. That these different effects apparently cannot be attributed to variations of density produced in the gaseous medium by the magnetic action, but very probably their explanation will be found in the perturbation induced by that action in the arrangement or disposition of the particles of rarefied gas necessary for the propagation of electricity."

It would appear the decrease of conductivity corresponds to the constrained position into which the electrical discharge is forced under the influence of the magnetism. The gases were inclosed in tubes to which was attached the necessary apparatus for measuring the variation in pressure.

It is well known that in working the tangent galvanometer with currents of high intensity, the tangents are not proportioned to the strength of the currents, owing to the needle being removed from the direct magnetic field. Professor Trowbridge, of Harvard College, has designed a galvanometer with which the results are more nearly proportioned. It consists of the ring and needle of the ordinary tangent galvanometer, the ring, however, instead of remaining perpendicular, being made to take any angle between the perpendicular and the horizontal planes. The deflection of the needle when the coil is horizontal being *nil*, the different angles of inclination give increased deflections. The deflection remaining constant, the intensities of the currents vary as the cosines of the angles of inclination of the coil. The instrument is therefore termed the cosine galvanometer. Many determinations of the intensity of the same current can be made by forming a table of the values of the cosines of the different angles of inclination in terms of the deflection of the needle which currents with a known interposed resistance produce. In the tangent galvanometer but one determination can be made. The delicacy varies inversely as the cosine of the angle of inclination. With large deflections, therefore, this instrument appears to give closer results than the ordinary tangent galvanometer; and used with a Gaugain's multiplier, very accurate estimations can be made.

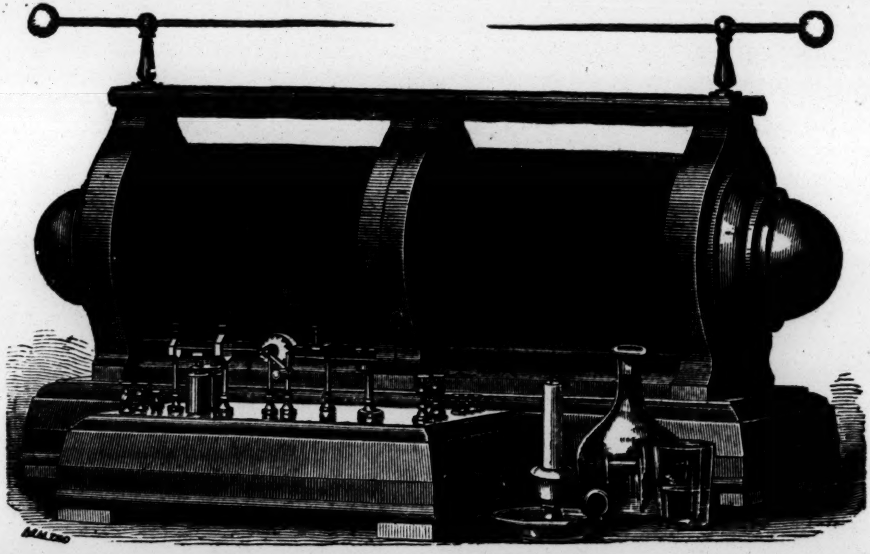
AMERICAN CORRESPONDENCE.

By PROFESSOR LEEDS, Stevens Institute of Technology.

Large Induction-Coil.—I have obtained from Dr. Wahl, the Secretary of the Franklin Institute, the following details of a very large induction-coil, probably the most powerful now existing, which has lately been constructed for Professor Morton by Mr. E. S. Ritchie, of Boston. Mr. Ritchie, as is well known, was the first, by several fundamental improvements, to make the induction-coil an efficient and reliable source of electricity; and it would seem that by constant attention to the subject, he has succeeded in making the most perfect instrument yet constructed. The coil now described, containing but $44\frac{1}{3}$ miles of wire, 40 inches in length, and weighing about 206 pounds, gives, with but three cells, sparks 21 inches in length, and, after several months of constant use and severe tests, is in perfect condition.

The accompanying woodcut, which is a faithful copy from a photograph of the coil, with some familiar objects as standards of comparison, will give a general idea of its structure and arrangement. It is made in three parts, one consisting of the condenser, enclosed in a mahogany case, as shown in the foreground carrying on its upper surface the automatic and hand-break piece, commutator, &c., and two others, forming the coil itself. These last are so arranged that they may be separated from each other, and used apart or united for quantity. The pole cups, by which the halves of the coil and condenser are united, have been omitted by the engraver on account of their confusing effect, as they were superposed by the perspective of the picture.

FIG. 21.



The particulars of construction are briefly as follows:—The iron core consists of iron wires, about $\frac{1}{16}$th inch thick, and weighs 14 pounds. The thickness of the wires is immaterial, except as it affects their annealing. These wires are not insulated from each other, and are simply bound together with a covering of oil-silk and cloth for strength.

The primary wire is 200 feet in length, and 0·1655 of an inch or about $\frac{1}{6}$th of an inch in diameter, and weighs 17 pounds. The secondary wire is 234,100 feet, or about $44\frac{1}{3}$ miles, and 0·07 inch in diameter, and weighs $44\frac{1}{2}$ pounds. It was made of Lake Superior copper, of the best electrical conductivity, and is covered with white silk. It is wound according to the plan devised by Professor Ritchie, in a series of spirals representing the thickness of the wire and its insulation, and has additional insulation of paraffin paper interposed at regular intervals.

The insulation between the primary and secondary consists of glass bells and vulcanite spools so proportioned as to offer the greatest resistance at points of highest tension, and proved by actual experiment to be 50 per cent greater than a spark of 21 inches would penetrate under the existing conditions. The condenser contains 325 square feet of tin foil insulated with oil silk, 100 square feet being in permanent connection with the primary circuit, and three buttons throwing on 100, 75, and 50 feet respectively at will. The break-piece is of the combined automatic and hand movement, attached by Mr. Ritchie to all his large instruments, the automatic break being operated by a single cell battery, connected or thrown out at pleasure by a button on the surface of the condenser-case. The total height to upper surface of horizontal strip is 18½ inches; total length of base from end to end of round caps over primary, 40 inches; height of base, 5 inches; width of base, 13 inches; length of each section of secondary bobbin, 13 inches; external diameter of secondary bobbins, 9 inches.

The battery for exciting this coil was made according to Professor Morton's direction, by Messrs. Chester Brothers, of New York, and consists of three glass jars, 10 inches in diameter and 12 inches high, into which are lowered by means of a windlass plates of carbon and zinc, 8 × 10 inches, five of each occupying each jar. The liquid employed is the mixture of potassic bichromate solution and sulphuric acid, now used in several forms of battery. When the solution is fresh, an immersion of 3 inches develops the full power of the coil.

In connection with a Leyden jar of 1⅓ square feet surface it produces sparks of 2¾ inches in length, and with one of Professor Morton's secondary condensers (see "Journal Franklin Institute," vol. liii., p. 256) shown in the accompanying cut (Fig. 22), and containing 20 coated panes, it gives sparks 14 inches in length, and of the intense whiteness and loud report of the Leyden jar discharge.

FIG. 22.

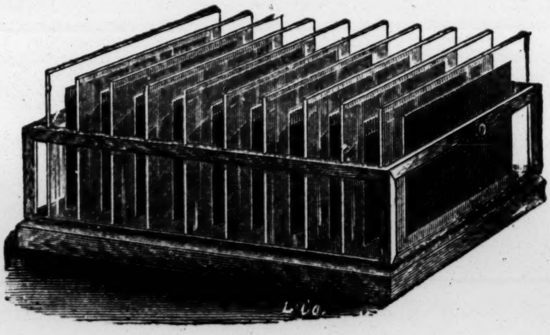
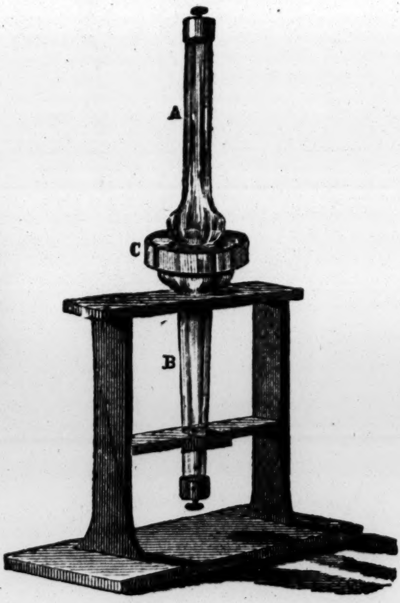


FIG. 23.



Blocks of glass 3 inches thick are penetrated, and seem to represent pretty accurately the same resistance as the 21 inches of air, for when the points are separated 21 inches, and other wires connected with the columns for piercing the glass, several sparks will pass in air before one with a red flash goes through the glass, then several sparks in air will occur before another spark will traverse the glass block again. Curiously enough, the spark in glass, as in air, seems to render its path a worse conductor than before, for it rarely happens that two sparks in a thick block go even partially by the same route, though the conducting points remain in exactly the same position.

The apparatus employed to pierce glass blocks is constructed as follows:—Two glass hollow columns, A and B (Fig. 23), are provided with five wires along their wires, the space about the wires being filled in with a mixture of wax and resin. These pillars are made very broad at one end, which is ground flat, and are provided with brass caps and binding screws at the other. They are cemented with wax and resin by the broad ends to the block of glass, C, to be pierced, the frame shown in the cut greatly facilitating the process.

Employed to illuminate a Gassiot's cascade when a secondary condenser of eight coated panes is in circuit, and an interruption or spark in air likewise interposed, the amount of luminosity is truly surprising, and greatly exceeds anything that we have seen with any other coil, even those made by Mr. Ritchie to give 15-inch sparks.

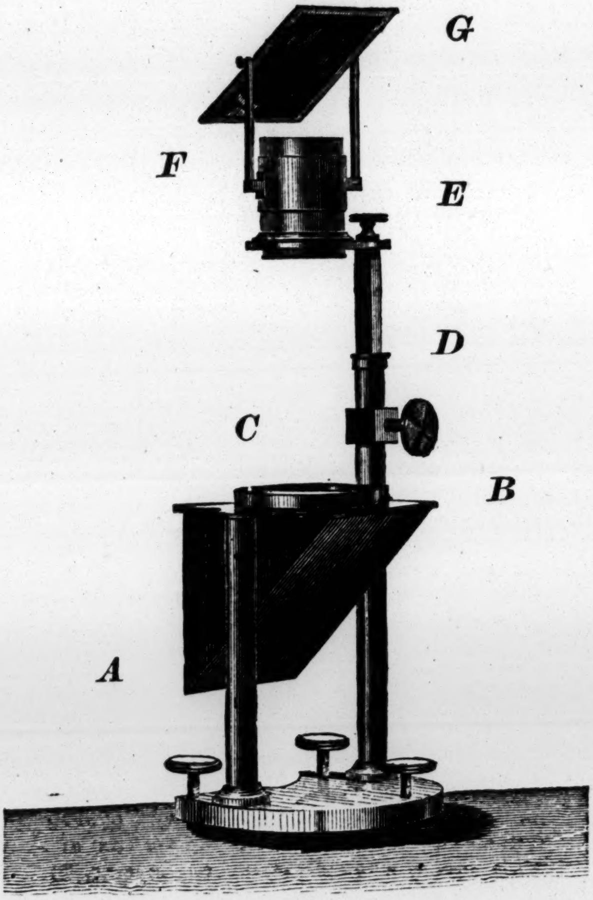
When coupled for quantity, the spark length is reduced to 12 inches, and the quantity is conspicuously increased, as is indicated by the sound and the aureola. When the poles are about 4 inches apart, this aureola may be blown into a flame-like surface, extending 3 inches from the line of discharge. Connected with a battery of four Leyden jars, the sparks are deafening, and afford light enough to illuminate a zeotrope disk 4 feet in diameter, so as to make the movement of its figures perfectly distinct at a distance of 60 feet. It is of interest to note in conclusion that a determination of the resistances made by Professor Morton gave for the resistance of the primary wire 0·13 of a unit (Brit. Ass.), and of the secondary wire 40,400 units.

Remarkable Electro-Magnet.—With the intention of attempting in conjunction with Professor Mayer to solve various outstanding problems in diamagnetism, in which unsatisfactory or negative results only have been obtained hitherto, Professor Morton has had made by Messrs. Wallace and Sons, of Ansonia, Connecticut, an electro-magnet of unusual form and of very great size. The poles of the core, which is made of the best wrought iron, are 3 feet 3 inches in height and 6 inches in diameter. They are hollow, the diameter of the bore being 3½ inches, and the thickness of the cylinder is therefore but 1¼ inches, which is a little greater than the thickness required for total electro-magnetic effect, as determined by Professor Mayer in accordance with the principles made known by him in a recent number of the "American Journal of Science." The diameter of the wire is one-fifth of an inch, and its total length is about 2000 feet. It is wound upon eight brass spools, which can be taken off or set on the core at pleasure. The weight of the magnet when complete is 1540 lbs.—that of each spool 112 lbs. It is proposed to employ three such galvanic batteries as were described in connection with the induction coil to supply an adequate galvanic current, and to connect them for quantity. This will give a total area of zinc surface amounting to 27 square feet. As yet no experiments have been made with a view of testing the performances of this mammoth magnet, but I hope shortly to present some account of them to the readers of this journal.

On the Projection of Magnetic Spectra, Cohesion Figures, &c., upon the Screen.—A very ingenious apparatus was recently exhibited before the American Institute by Professor Morton, for the projection upon a vertical screen of the images of objects, such for example, as waves in a tank of water, cohesion figures of various liquids, magnetic spectra, and the like, which can only be produced satisfactorily when the objects are maintained in a perfectly horizontal position. The original idea and general plan of the instrument shown was, as the speaker stated, due to Professor J. P. Cooke, of Cambridge, his own work in connection with it being confined to the devising of a convenient mechanical arrangement of parts, the improvement of the combination of condensing lenses with the reflecting lenses so as to secure a white and evenly illuminated field on the screen, and the discovery that an ordinary silvered mirror would serve for the final reflection as efficiently as a metal speculum or glass silvered by Foucault's plan, which are so difficult to obtain and keep in order. Faraday and Tyndall, the speaker further remarked, had employed an electric lantern turned on its back to throw images on the ceiling, and he himself had tried the same thing with a lime-light, and with a square prism had endeavoured to direct the rays on the screen, but with results unsatisfactory for reasons presently to be stated.

Mr. C. J. Woodward had also described a similar arrangement in the "Chemical News," vol. xix., p. 21. But last summer during a visit to Cambridge, Professor Cooke had kindly shown him in operation a lantern, in which the light was first thrown in a vertical direction by a mirror placed in front of the condensers, then passed through the horizontal object and object-glass, and lastly, was projected towards the screen, by a mirror silvered on its face. The only drawback to this instrument was, that the object being practically removed to some distance from the condensers, the field of light on the screen was shaded and discoloured.

FIG. 24.



The arrangement adopted by Professor Morton, and operating with such success that with it experiments were lately shown with striking effect in the Academy of Music, in Philadelphia (a building seating more than 3000 persons), was as follows:—

The lantern condenser in the first place is made of three lenses, the first two of such curves as to give with the light placed at about two inches from the nearer one a practically parallel beam. This beam is received upon a mirror, A B, placed at an angle of 45°, and after reflection from it falls upon the third lens placed horizontally at C. This concentrates it upon the objective at E, from which it passes to the mirror F G, and is so reflected to the screen. This mirror, moreover, is not silvered on the exterior surface, but in the usual way, though with pure silver on the back. Yet no want of definition is to be perceived in the image, owing no doubt to the fact that the faint reflection from the first surface is inappreciable in comparison with that from the metallic silver. In several articles published in the "Chemical News" and elsewhere, a square prism has been described as being used for the same purpose. But this arrangement leaves a third of the field dark, because about one-third of the cone of rays entering the prism is at an angle too great for total reflection. To exhibit magnetic spectra, a plate of glass is placed upon the third lens of the condenser, iron filings scattered evenly upon it, a small steel magnet placed beneath, and the glass lightly tapped with a pencil point. The various phenomena of wave motion, interferences, reflexions, &c., are demonstrated by Professor Morton, with an apparatus contrived by Messrs. Hawkins and Wale, the instrument makers, who satisfactorily carried out his instructions with regard to the making of the lantern itself. It consists of a metallic box with a sheet-rubber cover, provided with a long metal tube; this is so placed that the tube is about one quarter of an inch above the point in the tank which it is desired to make the centre of the wave motion. On tapping the rubber diaphragm a momentary puff of air is driven from the tube, producing exactly the disturbance needed. By placing an elliptical ring inside the tank, the reflexions, interferences, &c., are correspondingly modified.

By letting fall drops of ether, alcohol, carbolic acid, oil of cinnamon, coriander, cloves, &c., on the surface of water contained in the tank, cohesion figures are produced, which may be admirably exhibited in a lantern of this description, and so likewise the electric decomposition of metallic solutions.

Another experiment of peculiar beauty consists in attaching a ring of thin rubber, 5 inches in diameter, to a corner segment of a glass Chladni plate 12" square, and filling the ring about ⅛ inch deep with water. On vibrating the plate with a bow (the corner segment being of course in the lantern field), the area is filled with the most beautiful crispations, changing with the tone as the harmonics are sounded. This experiment, as we witnessed it in the Academy of Music, was of most unusual beauty.

Stevens Institute of Technology, U.S.,
July 15, 1871.

SCIENTIFIC INVENTIONS AT THE INTERNATIONAL EXHIBITION OF 1871.

SCIENCE is well represented at the present Exhibition, although the inventions and models to be viewed are not numerous. Each, however, either embodies the application of some highly scientific and known principle, or is the result of much thought and labour in a new field. There is an improvement in the arrangement this year much to be commended—the actual working of every model—rendering the scientific department not only more attractive to the uninitiated, but greatly assisting the comprehension of the invention. The general aspect of the Exhibition and Royal Albert Hall is well known from descriptions in the daily press; and we can at once proceed to chronicle that which is new in a technical sense. Messrs. Siemens, as usual, have contributed largely. The Pyrometer, by C. W. Siemens, F.R.S., has already been noticed in the pages of this journal. Dr. Werner Siemens exhibits two of his electrical inventions—the Electrical Distance Meter, an apparatus for obtaining a correct estimate of the position of distant objects, applicable more especially to the marking of the course of a hostile vessel approaching a battery of torpedoes, and to similar purposes. The other invention is the Dynamo-Electric Mine Exploder, the principle of which was described in a paper to the Royal Society in February, 1867. Mr. C. W. Siemens also contributes a model of furnaces for the production of cast-steel from iron ore, and a new Gyrometric Governor for steam-engines. Instead of the controlling pendulum hitherto employed, Mr. Siemens substitutes a parabolic cup dipping into water, and rotating about a vertical axis. Above the cup, the top and bottom of which are both open, are several vanes: the water, as the cup revolves, rises in the interior with the increase of rate, and, if the friction thus produced is not sufficient, overflows against the vanes, enforcing uniform velocity by a differential motion acting on the throttle valve of the engine.

Sir Joseph Whitworth contributes a measuring machine, a modification of that which obtained the Council Medal in 1851, capable of being read to the one-millionth of an inch. The principle is that of employing the sense of touch to aid that of sight, the gauge being made to pass between two perfectly true parallel planes, until the movement of the micrometer brings the surfaces so close that it is possible to feel the contact. This mode of delicate measurement follows from Sir Joseph Whitworth's invention of the method of obtaining a true plane surface.

Mr. J. Warsop exhibits an Aëro-Steam Engine, showing the application of the well-known fact that all water holds air in solution, and which acting as an elastic spring between the molecules of water, promotes ebullition. On this principle Mr. Warsop introduces, from an air-pump worked by the engine, a current of heated air into the water contained in the boiler. The air is heated by being forced through pipes placed in the flue or the smoke-box, thus utilising what has hitherto been waste heat. The air is passed into the water from a long perforated tube extending the whole length of the boiler, so that by the force of impact the whole body of water is constantly stirred and aërated. This agitation prevents the deposit of saline matter. Thus there is both a saving of fuel and a prevention of incrustation.

Messrs. Cooke and Sons exhibit a fine achromatic equatorial telescope (7435), of 10 inches aperture and 12½ feet focal length, with a clock in which there are arranged two separate trains of wheels driven by the same weight. One train is in gearing with a revolving fly, and also moves the telescope. The other drives an ordinary clock pendulum; this train having one wheel attached to an arm swung from a pivot on the frame, and shifting to the right or left with the pendulum. By this means the exact measurement due to a pendulum clock is obtained without any unevenness of movement.

Messrs. Elliott Brothers exhibit several new instruments. The Omnimeter, for surveying, consists of a theodolite telescope with a powerful vernier microscope. The telescope is pointed successively to two lines upon a staff of known length, and the arc described by the telescope gives the distance of the staff. The Planimeter, for estimating as accurately as possible the area of plans, resembles a large pair of compasses with a revolving disc at the joint of the legs, one of which is fixed, the other traced over the plan to be measured. The area is read off the disc, according to any scale to which the instrument has been set. Messrs. Elliott also exhibit Mr. Richards's steam indicator, which has been successfully employed in recording the variation in pressure of engines running at the highest speed.

Captain Le Boulengé contributes a chronograph for measuring very minute intervals of times. Two cylindrical bars, held up by electro-magnets, are allowed to fall freely in parallel vertical lines close together. One bar is of some length, and is sheathed with a covering of zinc; the other is short and drops upon a table almost as soon as released. In doing so, the blow struck upon the table liberates a knife, which indents a mark on the soft metal of the long falling bar. If the chronograph be applied to the measurement of the velocity of projectiles, the latter bar is set free when the bullet pierces the first screen, and the shorter bar when the bullet cuts the target. The distance through which the bar has fallen of course readily gives the time of the passage of the bullet.

Mr. T. E. Rowe exhibits a model of a Relume Signal Lamp, or a lamp which shall re-light itself automatically. It is well known that a compound straight bar will become bent by the action of heat, and will straighten itself again on cooling. Such a compound bar is fixed above the flame of the lamp, and a catch fixed to the end of the bar is held in place while the lamp is alight. So soon as the lamp is blown out, the bar begins to cool; when quite straight the catch is removed, liberating a spring which causes some matches to be ignited and light a second wick. This lamp is especially applicable to distant signal stations. Connected with the subject of light is the improved Jet Photometer of the late Mr. G. Lowe for the estimation of the illuminating power of coal-gas. It appears that when gas issues at a given pressure from an orifice of constant size, the height of the flame varies directly as the illuminating power of the gas. It amounts to the same thing if the height of the flame be kept constant, while the pressure is varied, ensuring a more delicate observation than could be made of the height of the flame. A table has been prepared showing the value of the light for the variable pressure as compared with the ordinary measure of sperm candles.

Messrs. Bergius and Whyte contribute the solution of a very difficult problem—the ascertaining of the depth of a marine sounding without any reference to the length of line attached to the sinking weight. As the pressure below the surface of the sea increases in proportion to the depth, means have been taken to record this pressure. The sounding weight is hollow, and a small tube communicating freely with the sea is carried nearly to the top of the apparatus. The compression of the air begins in this tube; the water soon rises to the top, and overflows into the chamber; there is no escape for the water so entering, and the measure of its amount, by means of a scale, shows the depth in fathoms to which the instrument has been sunk.

Mr. F. S. Duckham exhibits a hydrostatic weighing machine, constructed on the principle of the hydrostatic press, which will weigh up to 40 tons, though itself no larger than an ordinary spring balance. The object to be weighed is hung upon a piston, fitting in a small cylinder, and pressing upon the upper surface of some water; the amount of pressure, measured by a gauge, readily gives the weight.

Before leaving the building everyone passes to the enclosure in which Mr. Hodgson's wire-tramway is at work; this seems to be taking up and delivering its loads as indefatigably as when on the Brighton Downs, where the public first became acquainted with the results that can be achieved by this invention.

In this short notice it is impossible to enumerate all the improvements exhibited; the endeavour has been to show that although many complaints have arisen as to the paucity of scientific invention, they are certainly combated by the consideration of the value of the exhibits. The admirable reports of Professor T. M. Goodeve, M.A., Professor Abel, F.R.S., Lieutenant T. English, R.E., and Henry Sandham, Esq., on the several branches of science, will fully bear out this view.

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